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NOTES AND COMMENTS.

"—OLOGISTS" *versus* "COLLECTORS."

CONTRIBUTORS to our contemporary, *The Entomologist*, have lately been discussing the somewhat academic question whether one who does "not make his collecting entirely subservient to the elucidating of scientific problems" is or is not" worthy of the name of Entomologist." The answer obviously depends on what one means by "entomologist." If entomology is, as its derivation implies, the "science of insects," then an entomologist must be "a scientific student of insects"; and one who collects insects for the sake of turning an honest penny, or of putting them in picture-frames over the mantel-piece, or of quietly gloating over their beauty as a miser over his gold, or even—still as a miser—of making a larger collection than his neighbours,—such a one may be an excellent father or a model parishioner, but is assuredly no entomologist.

The discussion, however, has not been altogether barren. The fact has been insisted on, just as we have of late been forced to insist on it, that a collector need not necessarily be possessed of great erudition to enable him to do honest and useful work. The field-naturalist and the amateur are in a far better position for doing valuable work than is the average professional scientific man—tied to a museum workroom or a lecturer's table. But, unfortunately, the collector in so many cases does not do good work: he collects without observing; he cares for the name, not for the nature, of his newest find. It is the collector to whom the museum and laboratory naturalists look for valuable and essential information concerning the material with which they have to deal; but when they so often look in vain, can we wonder at an occasional grumble?

We have been accused of late in various quarters, by individuals and by some publications whose opinion we value, of attacking systematists, of attacking field-geologists, of attacking amateurs, of attacking state-paid officials, of attacking collectors, of attacking

philosophers, of attacking morphologists. We know of no ground for these accusations. What NATURAL SCIENCE is founded to attack is *bad work*, what it has always laboured to defend and promote is *good work*. The day on which this Review ceases to distribute praise and blame, wrongly perhaps, but without fear or favour, will be the day on which it ceases to exist.

THE SCIENTIFIC ARRANGEMENT OF INSECT COLLECTIONS.

ANOTHER good result of the discussion in *The Entomologist* has been the appearance in the October number of that journal of a paper by Mr. W. Harcourt Bath, entitled "Should the formation and arrangement of a collection of insects be made subservient to the elucidation of scientific problems?" For his answer the author describes the plan of his own collection. The two subjects to which he rigidly confines his study are Distribution and Variation, with special reference to the Rhopalocera, European and Exotic. These subjects can be studied side by side, and the formation and arrangement of the collection made with reference to the two at the same time. At present attention is confined to the two zoological subregions in which Europe is included, and these are divided further into provinces and sub-provinces, which again are divided vertically into climatological zones. Mr. Bath's aim is to obtain a series of every species possible from each of these subdivisions, the series to be first typical, and then illustrative of variation and seasonal dimorphism. For the elaborate and ingenious method of arrangement and labelling employed, we must refer our readers to *The Entomologist*; and we need only quote the author's words, "it affords me much pleasure, from an intellectual point of view, which I did not dream of when I contented myself with being a 'mere collector.'"

The general question raised by Mr. Harcourt Bath is of such wide and practical interest that we hope room may be found in our contemporary for further discussion of it. It is clear that specimens of any kind, arranged in some logical order, are more likely to elucidate problems than those arranged on no scientific plan; moreover, the superiority of specimens to elaborate descriptions, even to tabulated statements, is apparent at a glance. Evidence of the value of Mr. Bath's method is afforded by his paper in the November number of the same journal "On the vertical distribution and derivation of the Rhopalocera in the Pyrenees." Of course each individual collector will have his own predilections, problems, and arrangement; the more diverse their points of view, the better. But when the collections of public museums come to be considered, the case is altered. We do not mean that they should have no scientific arrangement; but it seems to us that in each case the arrangement must primarily be systematic. At the British Museum the Lepidoptera are arranged according to a classification based on the latest

views as to their genealogy. Here, however, we enter on a wide problem that might well be discussed at this year's meeting of the Museums Association; for it is to be at Oxford, where Professor Poulton and his energetic helpers can give a practical demonstration on the Hope collection.

The Kind of Work we Want.

Considering the strange misconception that our views have met in certain quarters, we almost despair of being able to explain what kind of work it is that we really should like to see. But in this very subject of Entomology, here is one of our best observers of nature, who has come in the nick of time to help us out of our quandary. In the well-filled January number of *Knowledge* is a paper "On describing and drawing insects" by Mr. F. Enock, whose admirable advice we recommend *mutatis mutandis* to others than would-be entomologists. Keep a *daily* journal of observations upon insect life, noting down in black and white every fact connected with the economy and habits of such insects as you are studying, or as pass under your observation. "Owing to the irregular methods which some authors adopt in their descriptions of insects [and other organisms], the task of naming is made more difficult. No two insects are described in the same order. Head, thorax, abdomen, legs and wings, in one; then in the next some other plan, making comparison of parts most confusing." To obviate this, draw up, and place beside you when describing, a definite order of description. "Whenever possible, make out your descriptions from *living* insects, when so much more *purpose* can be seen in the various parts, many of which contract and dry up out of sight in a dead specimen. I often think that much of the dryness or dulness of descriptive entomology is attributable to the fact that it has been made from lifeless creatures . . . The *pose*, too, of the antennæ, head, and whole creature can only be observed in a living insect; the mysterious light in the eyes and the ever-changing prismatic colours on the gauze-like wings, all can be seen, and all appeal to our susceptibilities." Make an *exact* drawing under an inch objective, not a freehand drawing or one in which you can put in any of your own ideas. "All drawings should be carefully inked in and the scale marked under each."

If everyone worked in this intelligent and accurate manner, half our occupation would be gone.

The Earthquake.

Whether we consider its disturbed area or the damage inflicted by it on buildings, the earthquake of December 17 will rank as one of the most important ever recorded in these islands. In Northumberland and Durham, several persons believe that they felt it; but, even if these two counties eventually should have to be excluded, the

disturbed area embraces all the rest of England, the Isle of Wight, the Isle of Man, the whole of Wales, and overlaps the eastern counties of Ireland. The exact extent is at present undetermined, but it is clear that in this respect hardly any known British earthquake can surpass it.

The damage to property, though in no place of great consequence, is far from inconsiderable, being spread over rather a wide area. The city of Hereford appears to have suffered most. Several of the pinnacles of St. Martin's Church were broken, aud one of the pinnacles of the Cathedral was displaced. The chimneys of many houses were thrown down, and the walls of others cracked. At Gloucester a few chimneys were overthrown, and reports of similar slight damage come from Worcester, Dursley, Ross, Cinderford, and other places in the surrounding country.

The strongest shock occurred about 5.32 a.m., but it was preceded and followed by several slighter ones, of which unfortunately there are very few observations, owing to the early hours at which they occurred.

Professor Milne states that two of his delicate seismometers in the Isle of Wight show slight but certain traces of the earthquake, the movements continuing for one or two hours. At the Kew Observatory, a faint record is exhibited by the declination curve, and a more distinct one by the horizontal force curve; but the magnetographs at Greenwich and Stonyhurst were not affected. All of these places, however, lie within the disturbed area, and no record has apparently been obtained from any instruments outside it; the nearest one capable of affording any evidence is the horizontal pendulum erected a few years ago at Strassburg.

It seems very probable, as suggested by Dr. C. Davison, in *Nature* (vol. lv., p. 179), that the recent shocks may be connected with an earthquake which occurred about thirty-three years ago, on October 6th, 1863. The latter was carefully studied by Mr. E. J. Lowe, F.R.S., and was but little inferior in intensity and extent of disturbed area to the shock of December 17th. The part of the country where it was most severely felt is bounded by a line passing a short distance from Worcester, Leominster, Brecon, Newport, Monmouth, and Ross. It "was felt throughout Wales and the central counties of England, extending north as far as Doncaster, Huddersfield, and Clitheroe; east to Market Rasen, Peterborough, and Bedford; south to London, Dorchester and Plymouth; and in the west crossing St. George's Channel to Dublin and Wexford:" though it was also perceptible at several isolated places beyond these limits, such as Lancaster, Ulverston, Harrogate, Malton, Scarborough, Bury St. Edmund's, Brighton, and the Isle of Wight.

Dr. Davison is investigating our latest earthquake, and would be glad to have details of any observations sent to him at 373 Gillott Road, Birmingham.

Professor Bonney on Alpine Geology.

The history of geology has been described as a succession of nightmares. Thanks to the extreme difference of the methods employed in the different branches of the science, the experts on one part of the subject, if exclusively specialists, are often quite unable to understand the work of their colleagues in another. The crystallographic formulæ of the petrologist are usually as unintelligible to the palæontologist as a page of Chinese. Hence theories proposed by men of eminence in geology often become articles in the orthodox creed of that science simply by the subscription of men who are quite unable to check the methods or reasoning on which the theories are based. Thirty years ago a plague of theories was fastened on the shoulders of geology by that Old Man of the Sea, Eozoon, owing to the advocacy of biologists who regarded chrysolite as the same thing as chrysotile. Some years later came the great "schist question." According to what was once the orthodox school, schists were rocks produced by the alteration of sedimentary rocks belonging to various ages, Palæozoic, Mesozoic, and even Tertiary. When schists were found sandwiched between fossiliferous rocks they were all regarded as a series of beds of the same age, of which one layer has been altered and the other left. Accordingly the present school of geologists has grown up saddled with a load of theories proposed to explain away the eccentricities of "selective metamorphism." These theories have been especially disastrous to geological work in mountain areas. Instances quoted in support of the fashionable view from Nubian deserts or from South American jungles, or based only on single traverses, of complex mountain groups, carried no weight. The theory rested in the main on the " Mesozoic schists " of the Alps. All the leading Swiss geologists accepted the theory ; they coloured the official maps of the Swiss survey in accordance with it, and taught it in the bulky monographs that accompanied those maps. It thus had an immense influence on the geological thought of the rest of Europe. When sceptics doubted they were referred to the *Beiträge zur Geologischen Karte der Schweiz*, and if they adduced facts inconsistent with the views there promulgated, they were told that it was so much the worse for the facts.

During the last few years, there has come a complete change of view as to the age of the Alpine schists, and a consequent change in the interpretation of the whole geology of the Alps. The geologist to whose teaching this revolution is mainly due is, unquestionably, Professor T. G. Bonney, who on New Year's Day gave the Geologists' Association, London, a summary of the results of his 27 visits to the Alps. This lecture will no doubt shortly be published by the Association, and our object here is to direct attention to what is sure to be a clear and reliable outline of the fundamental facts of Alpine geology. It was not to be expected that the Swiss geologists would immediately admit the overthrow of the theory, which was once almost universally

accepted from faith in their work. It was clear from remarks made at the conclusion of Professor Bonney's lecture that some English geologists have not appreciated how utterly the theory of the Mesozoic age of the crystalline schists of the St. Gothard area has been demolished by Professor Bonney's paper of 1890. To that paper no serious reply has ever been attempted. In fact, from the long list of literature on the St. Gothard area, published in the "Livret Guide" prepared by the Swiss geologists for the Geological Congress of 1894, that paper was carefully excluded. The failure of the Swiss geologists to accept the challenge there made to them to substantiate the accuracy of their observations, has relieved geology from an incubus of error, for release from which Professsor Bonney deserves the thanks of all geologists.

SCHISTOSITY AND SLATY CLEAVAGE.

MR. G. F. BECKER'S paper on this intricate subject in a recent number of the *Journal of Geology* (vol. iv., pp. 429–448) is a lucid non-technical presentation of his theory of these rock-structures, as more fully elaborated in an earlier memoir. Of the different kinds of deformation to which rock-masses may be subjected, the author considers specially two distinct types of strain, which he names "pure shear" and "scission," though in general the effects of the two must be associated. In the former the directions of the planes along which flowing movement takes place are continually changing; in the latter one of the two directions of flowing is fixed. The author further considers the effect of viscosity as superposed on that of elastic resistance and as modifying the results. He submits that the resistance of the rock to rupture must necessarily be modified along those planes which have, during the process of deformation, been planes of flow; and he decides on experimental grounds that they will be planes of weakness rather than of increased strength, or, in other words, they will be planes of schistosity or cleavage.

On this hypothesis, pure shear would give rise to structures parallel not to a definite plane, but to planes lying between two limiting planes, making a certain angle with one another which would increase with the amount of shear; and there would be two sets of such planes, equally inclined to the direction of maximum compression. In the other ideal case, that of pure scission, one set of planes would be strictly parallel, while the other set would range through an angle twice as great as in the former case for a like amount of deformation. The author considers, however, that, owing to viscosity, the latter set would usually not become effective. The argument here is not convincing, for if viscous resistance be proportional to the *rate* of shearing (as it is in a homogeneous substance), and if this rate be extremely slow (as is usually presumed), this part of the resistance may be a vanishing quantity at every point of time.

We incline to think that any investigation which treats rock-masses as homogeneous substances must of necessity be unsatisfactory. The commonly accepted theory of the cleavage-structure has for its starting-point the heterogeneous structure of the rock affected. Differing as to postulates, Mr. Becker's theory also differs in its conclusions from the commonly received one. He finds that the structures induced must stand at acute angles to the causative force, instead of being perpendicular to it. This is very difficult to reconcile with the evidence presented by rock-masses in the field. It is also inconsistent with the evidence of the deformed concretions, fossils and the like, in numerous slates, which give us an actual picture of the strain-ellipsoid; in these cases the cleavage plane is always perpendicular to the least axis of the ellipsoid. While the paper is in itself of much interest, it may be regretted that the author has not made a fuller comparison between his theoretical conclusions and the facts of observation.

The Classification of Precambrian Rocks.

There has always been a tendency for men to classify natural facts, not according to knowledge, but according to ignorance, just as we even now continue to define the genera of living beings by the breaks in the chain, or the periods of geology by the gaps in the record. It is as a consequence of this human frailty that the history of geological science shows us a continued attempt to separate certain of the oldest rocks from their successors on the ground of the absence from them of fossils, and to label them Azoic or some equivalent name. As our knowledge grows the limits of this primitive group of rocks become more restricted; so that from having included all strata below the Devonian, it now includes only comparatively few of the ancient crystalline rocks. But the want of logic in this division is shown not only by continual fresh discoveries of fossils, but by the assumption, nowadays universal, that the oldest animals known to us were preceded by a long array of ancestors. Acting, no doubt, on this conviction, at a recent meeting of the Geological Society of Stockholm, Dr. J. J. Sederholm, head of the Geological Survey of Finland, pointed out how difficult it was to draw a true line of demarcation between the strictly preorganic or azoic rocks and those that possibly were once fossiliferous. He would designate the whole basal complex as "Archæan," without reference to contained organisms, separating from the succeeding "Archæozoic" on the ground of its having been affected by certain earth-movements of earlier date than the deposition of the latter rocks. The Archæozoic rocks again, for the Scandinavian region at least, Dr. Sederholm would divide into the older, "Jatulic," which have been involved in certain great mountain-folds, and the younger, "Jotnic," which are later than these folds.

TELEGONY AGAIN.

SOME time ago in our own columns (NATURAL SCIENCE, vol. iii., p. 436), Mr. Frank Finn brought together all recorded cases he could find, of which the most probable explanation appeared to be the occurrence of telegony. The collation was interesting and useful; but the problem was left very much in its previous condition. The majority of breeders are assured that the influence of an earlier mate is frequently seen in the progeny of a mother to a second mate; they take the greatest care to avoid accidents of this kind, believing that a pure-bred dam is spoiled for stock purposes if she has been crossed by a mongrel or hybridised. On the other hand many great breeders, and, perhaps, the majority of scientific naturalists, decline to admit the existence of any evidence for telegony, while some even deny the possibility of its occurrence. It is, of course, impossible to prove a negative, but the scientifically conducted experiments for which all naturalists must wish, have not yet been made on any satisfactory scale. Much is to be hoped from Professor Cossar Ewart's experimental stud. As most of our readers are aware, Professor Ewart succeeded last year in effecting a cross between a zebra stallion and Mulatto, a West Highland pony. The hybrid is strongly marked with stripes, although these differ from the stripes of the sire. Since the experiments are to be continued for some time, definite results may be looked for. The first interesting event that is expected is the birth of another foal from the same dam, the progeny of a horse. If this occur without mishap, telegonic influence of the zebra will be looked for.

In the meantime, Mr. Karl Pearson has presented to the Royal Society an investigation into telegony from another point of view. It occurred to him that if a telegonic influence existed, there might be a closer correlation between the younger children of a family and their father, and a lesser correlation between the younger children and their mother, than existed in the case of the other children. n fact, it might be an accumulating influence on the body of the mother by the father. With this possibility in view, he examined a large series of family statistics, and found that so far as stature was concerned, the result was negative: that there was "no evidence whatever of a steady telegonic influence of the male upon the female among mankind."

A third recent discussion of telegony, is that already reported by us as having occurred at the meeting of the Zoological Society on November 27th, when Mr. Chalmers Mitchell exhibited on behalf of Mr. Oswald H. Latter, a fox-terrier puppy whose sire was a fox-terrier and whose dam was a fox-terrier that had thrown a litter to a dachshund in the preceding season. There was no question that the puppy was a disgrace to well-bred parents, but Sir Everett Millais, who himself has conducted a large series of experiments, was convinced that it was a case of reversion to the beagle strain, which is part of

the history of fox-terriers, and refused to see in it any relation to dachshunds. Obviously, the difficulties of getting evidence that shall exclude reversion, are very great in strains so sophisticated as those of modern dogs; but in so doubtful a subject, the investigation of every possible case is advisable, and we are glad to learn that Mr. Latter has set going further experiments on the subject.

The Function of the Intercostal Muscles.

The precise action of the intercostal muscles has been the subject of much dispute amongst physiologists. The simple and diagrammatic arrangement of hinged rods connected by crossed indiarubber bands, which Huxley's "Elementary Physiology" has rendered familiar to us from our youth up, has been recognised as a somewhat incomplete representation of the complex movements of the ribs in respiration. Some writers have declared their belief that the intercostal muscles play no important part in that act, and indeed so recently as 1894 Weidenfeld, as the result of experiments on dogs, maintained that they remained completely passive even in extreme dyspnœa. It is, however, admitted by almost all physiologists that the external intercostals and the intercartilaginous portions of the internal intercostals are inspiratory in function, acting synchronously with the diaphragm, though Masoin and Du Bois-Reymond found that, in the case of the intercartilaginei, this was only the case in forced inspiration. The action of the main mass of the internal intercostals, on the other hand, has been much less certainly recognised; they have been claimed as inspiratory muscles, though the opinion has been gaining ground that they are expiratory in function. Martin and Hartwell have in recent years maintained this, and indeed the fact that they contract alternately with the diaphragm makes it highly probable.

During the past year Messrs. K. Bergendal and P. Bergman published in the *Skandinavischen Archiv für Physiologie* (vol. 7, pp. 178–185) the results of a series of experiments carried on by them in Stockholm, on the action of the intercostals in rabbits, dogs, and cats. These seem conclusively to show that, in these animals at least, respiration can be carried on by the intercostal muscles alone, after the extirpation of all other respiratory muscles except the triangularis sterni, and that while the external intercostals and the intercartilaginous portions of the internal intercostals are inspiratory in their action, the interosseous portions of the latter are no less clearly expiratory. The experiments were carried out on narcotised animals, and tracheotomy was performed as a preliminary in case artificial respiration became needful during the course of the experiment. On exposing the external intercostal muscles and severing them from their lower attachments to the ribs, they were seen to contract visibly during inspiration, and the same was true of the intercartilaginei.

On isolating, after Rosenthal's method, a section of the chest-wall, including the second to the fifth ribs, on one side, so that it was completely separated from the ribs above and below and from the sternum in front, and on dividing all the muscles connecting this section with the vertebral column, so that the only muscles which could act on it were the intercostals, it could be seen that the ribs in the isolated section were strongly raised at each inspiration, the movement ceasing when the intercostal nerves were divided. Further, when the external intercostals were divided in this section, leaving the internal intercostals intact, the ribs were passive in inspiration, but were drawn down in expiration; this movement ceased when the internal intercostals were also divided. In a still more elaborate series of experiments the authors succeeded, in cats, in extirpating the entire musculature of the chest-wall with the exception of the intercostals and the triangularis sterni; even the intercartilaginei were severed. The phrenic nerves were divided. Pneumothorax occurred in two of the experiments; in two it was successfully avoided. In all cases active respiratory movements occurred, brought about solely by the action of the intercostal muscles, and it was clearly manifest that the external intercostals raised the ribs, while expiration was not merely a passive recoil, but was produced by an active contraction of the internal intercostals. In one case the animal breathed quietly and peacefully for ten minutes after section of the phrenic nerves.

These very interesting and complete experiments should set at rest the question of the action of the intercostal muscles during forced inspiration and dyspnœa. It is perhaps not absolutely safe to argue from cats and rabbits to man, but the disposition of the muscles is substantially similar in man, and no good reason can be shown in this case for not so arguing. It may not be quite safe to interpret the phenomena of quiet respiration under normal conditions by the light of the facts observed during the dyspnœa of such severe and abnormal conditions as have been above described. But it is incredible that muscles which have one definite action in dyspnœa should have a different action in calm breathing, and there is at least a strong presumption that their action is similar in character, though less in degree. And so, after years of wandering, we come back to the old familiar hinged rods and rubber bands, glad to find that, after all, they were very near the truth.

A New Giant-Bird.

Some years ago it was announced by Professor Stirling that a number of skeletons of *Diprotodon* and other extinct marsupials had been discovered exposed on the surface of the ground at Lake Callabonna in South Australia, and that the remains of a large extinct struthious bird occurred associated with these mammals. Further details of this remarkable find have been awaited with some im-

patience, and it is only within the last few months that there has appeared a paper by Stirling and Zietz (Preliminary notes on *Genyornis Newtoni*: a new genus and species of Fossil Struthious Bird found at Lake Callabonna, South Australia: *Trans. Roy. Soc., South Australia*, vol. xx.), which gives a brief account of this interesting bird. It is extremely unfortunate that no figures are given, since in the case of bird bones mere description and measurements convey little idea of the structure of the specimens.

It appears, however, that the femur and tibia of this bird are very similar in size and proportions to those of the Elephant-footed Moa (*Pachyornis*), but that the metatarsus is longer and more slender than in that bird. The inner toe was slender, while the middle and outer ones were short and flattened, their ungual phalanges being particularly small, short, and flat; one very peculiar character is the presence of only four instead of the usual five, phalanges in the outer toe. The sternum is said to resemble that of the emu, and the wing, though greatly reduced, was complete. The skull is nearly a foot long, and the mandible remarkable for the depth of its rami and the wide angle they include between them.

The authors, in conclusion, state that *Genyornis* probably lived in marshy places, and fed on herbage like the emu, which in many respects it resembled. It is to be hoped that further description and figures of this interesting new form will be forthcoming before long.

Monkeys in Madagascar.

One of the most important results of Dr. Forsyth Major's travels in Madagascar has been the discovery of the remains of an extinct monkey-like animal in the superficial deposits of that island. Dr. Major has briefly described and figured his find in the *Geological Magazine* for October, the relics consisting of portions of a skull and lower jaw, and has called the animal *Nesopithecus*, believing it to be a true monkey, indicating a new family of the Anthropoidea, "intermediate in some respects between the South American *Cebidæ* and the Old World *Cercopithecidæ*, besides presenting characters of its own." Mr. Lydekker, writing in *Nature*, November 26th, however, rather doubts its being an anthropoid, and suggests it may be "an offshoot from the original stock which connected the Monkeys with the Lemurs."

The discovery obviously is one of the highest importance, and we shall be interested to read Dr. Major's full description and examine his figures and conclusions when they appear. Till then we offer no opinion.

The Range of the Nummulite.

The already wide range of the genus *Nummulites* has been still further extended. In a short note to the *Geological Magazine*

(November) Mr. R. B. Newton has recorded the occurrence of this genus in rocks from Gazaland, South East Africa. The specimens come "from a large development of flesh-coloured limestone," but further specimens and evidence are desirable. We have now, therefore, suggestions of a much more extended range of the Nummulitic limestones of Eocene age, viz.:—England to China, England to South East Africa, China to South Australia, in the Old World; the West Indies and United States in the New World. In vertical range the genus is first found in Carboniferous rocks; there is a record from Cretaceous, but the main development of the genus was in the Eocene period, after which it seems to have died out.

Rank and Quality, and the Royal Society in 1778.

In the correspondence of William Legge, second Earl of Dartmouth (*Hist. MSS. Comm. 15th Report*, App., Part 1.) appear two letters touching the Presidency of the Royal Society, which contain matter of sufficient interest for quotation here. The first was written by Dr. Charles Morton and dated "British Museum, 25 Sept. 1772" and is quoted as follows:—"By direction of Mr. Barrow, President of the Royal Society, and many of the members, he writes to ask Lord Dartmouth to allow himself to be Proposed as President. The office includes a Commissionership of the Longitude, a Trusteeship of the British Museum, and the first Visitorship of the Royal Observatory."

The second is from Nevil Maskelyne and is dated "Royal Observatory at Greenwich, 22 Sept. 1778." It reads:—Your lordship is doubtless apprised, were it only by the public prints, that Sir John Pringle means to resign the office of president of the Royal Society on the Anniversary election at St. Andrew's day next. I can add, that, about a fortnight ago, he declared that resolution at a council which he had summoned on purpose, without any person being then mentioned either by himself or any other member of the council, as eligible to succeed him. It was, doubtless, his intention not merely to pay a compliment to the council, but rather to give the members an opportunity of enquiring after a proper person to fill the President's chair, and to make an opening for some person of distinguished rank and character, like your Lordship, to offer their service to the Royal Society on the day of their Anniversary election. There is no person, who publicly declares himself as aspiring to the Chair; so that it is entirely open; and from conversations which I have had with several fellows of the Society on the propriety, and even necessity, of prevailing on some nobleman to honour us by accepting the office of President, it appears to me that they are waiting for such an event, which would at the same time do most honour to the Society, and be most advantageous to science; for, without rank in the chair, the Society itself will not be so much

respected and obtain that *éclat*, either at home or abroad which so useful an institution deserves; nor will the great end of the Society, the promotion of science, be carried on with so much vigour, since it is well known, that without the protection of the great, the arts and sciences will always fade and languish. Your Lordship, I flatter myself, will not be surprised, after what I have advanced, not without good grounds, of the sentiments of a great part of the Royal Society, at my declaring myself one of many who earnestly wish your Lordship would do us the honour to give us leave to mention your name to the fellows of the Society at large, and canvass them, in order to your nomination to be elected President of the Royal Society on November 30th next. At present (although I have mentioned your Lordship's name at a distance, to many fellows of the Society, and found it always received with the utmost respect and attention), yet, out of regard to your Lordship's delicacy, I have only apprised a very few gentlemen of this address to your Lordship, viz., Mr. Daines Barrington, Dr. Shepherd, Mr. Walsh, Dr. Warren, and Mr. Wegg, Vice-President and Treasurer of the Society. We are fully satisfied, that if your Lordship would but permit us to name you to our Brethren of the Society, even without your Lordship appearing at all in it yourself, if you did not choose it, that there would not be the least difficulty or obstacle to the gratification of our wishes, and the fixing your Lordship in the President's chair on the day of the election. We are the more induced to indulge our hopes that your Lordship will honour us by acceding to our wishes, from the consideration that your Lordship once actually accepted the office of President before Sir John Pringle was nominated, and only declined afterwards on account of the engagements of a busy office which your Lordship then held—that of Secretary of State for the American Department, which however having long since resigned, your Lordship is now at leisure to accept an honourable office with some share of business. We expect, in about a twelvemonth, to take possession of the new and magnificent apartments which his Majesty has been pleased to give us in the new erection on the old site of Somerset House, which will be more agreeable to your Lordship, as well as to the Society, than our present incommodious crowded house; and where we may expect to have our meetings attended by more persons of rank and quality than heretofore. If the usual hours of the meettings of the Society should be less convenient to your Lordship, they might be altered either to a later hour, or to one before dinner, and where it might not be convenient to your Lordship to attend at all, the chair might be taken for the time by a Vice-President, who is always of the President's nomination."

It is interesting to note that the chief bait held out to the proposed President is the "rank and quality" at the meetings; and the declaration of Maskelyne, one of the foremost men of his time, that science then depended, as art always does, on the patronage of the great.

SOME AMERICAN BOTANY.

THE seventh annual report of the Missouri Botanical Garden which has just reached us contains, besides information about the gardens, several scientific papers. In fact, the garden-report ends at page 24; the rest of the book (which contains 209 pages) being occupied with papers dealing with the botany of the United States, a short address on the value of a study of botany, and a catalogue of a collection of books on pre-Linnean botany, the generous gift of Dr. Lewis Sturtevant. The plates are, as usual in these reports, excellent. There are no less than sixty-six to illustrate the three scientific papers, and also half-a-dozen depicting various plants of interest which have flowered during the year. Among these we note the *Victoria regia*. As the flowers of this great water-lily expand in the twilight the garden was on several occasions kept open in the evening to enable those who wished, to witness this interesting function. Professor Trelease, the director, contributes an account of the *Juglandaceæ* of the United States, in which special attention is drawn to such characters of the fruit, twig, bark, and bud as will be helpful in field studies. So well-marked are some of these characters that in the case of the hickory "most of the species are more readily known in their winter condition than during the period of flowering." The same is true of the walnuts. Some excellent photographs of portions of the trunks of different species give a good idea of the appearance of the bark, which shows marked differences according to the species. Miss Mulford contributes a descriptive revision of the agaves of the United States. Most of the agaves have their home in arid deserts of Mexico, Central America, and the south-western portions of the States. Owing to the rarity with which they bloom under cultivation they have earned the popular name of "Century plant." In their own country they have been used by man from time immemorial. The Aztecs showed their appreciation by reverencing the plant as a god. Saddle-cloths, sacks, and ropes have been made from the tough fibres, and Miss Mulford quotes, on Humboldt's authority, "a bridge at Quito, having a span of 130 feet, made of ropes of agave fibre four inches in diameter." The softer parts supply food and drink. Pulqué, a universal drink in Mexico, is made by allowing the sweet sap of the best-known species, *Agave americana*, to ferment in vats of raw hide. This species blooms at ten years of age. When it is about to send up its large flowering-stalk an enormous flow of sap takes place towards the central bud to meet the necessary expenditure. The Mexicans and Indians cut out the bud and surrounding leaves and collect the liquor, which flows freely from the wound, in a long cylindrical gourd. "Some plants produce an average of two gallons a day, and will keep up the supply for months." The leaf-spine with attached fibre serves as a ready-threaded needle; the strong flowering-stalks make handles for lances, poles for fishing, walls for houses; and it is said that the juice mixed with wall-plaster

is used as an insecticide to keep away the destructive white ant. Its spiny leaves render it a very effective hedge-plant, and it has been extensively introduced for this purpose in the Mediterranean region.

A third paper is a short account of native ligulate Wolffias, small fresh-water plants closely allied to our duck-weed, by C. H. Thompson.

The Occurrence of Marine Organisms.

Many of the organisms commonly found in the plankton of the sea around the British Coast exhibit remarkable variations in their relative abundance at particular localities from year to year, but little is known as to the extent and causes of such variations. As a number of naturalists make use of the tow-net at many places round the coast, especially during the summer, much valuable information would be obtained, if in all cases records were kept of the presence or absence of a limited number of the commoner species, and these records subsequently brought together.

The Director of the Marine Biological Association of the United Kingdom has taken this subject in hand, and desires to tabulate the results before January 31st, 1898. The following is a list of the forms upon which information is desired, and it is followed by the nature of the information asked for:—*Halosphaera viridis*; *Noctiluca miliaris*; *Aurelia aurita* (including *Ephyrae*); *Agalmopsis*; *Muggiæa atlantica*; *Hormiphora plumosa*; *Beroe*; *Tomopteris*; *Anomalocera patersoni*; *Doliolum*; *Salpa*. Where the generic name only is given in this list, the specific name of the specimens taken should be added. Should any doubt exist, preserved specimens should be kept. In making a record the following should be stated:—Date; Hour; Locality, (with as much accuracy as possible); Depth, (depth of water, and maximum depth at which net has been worked); Quantity, (0, absent; 1, few only; 2, moderately plentiful; 3, exceptionally abundant). Observations on the temperature of the sea, and notes on wind, tide, &c., will also be of value. Records should be sent in before January 31st, 1898, or forwarded from time to time to the Director, Marine Biological Association, Plymouth.

The Problem of a Parasitic Copepod.

Let the impatient reader purchase 500 specimens of the Norway Lobster. Mr. J. Stewart Thompson (*Proc. Phys. Soc. Edinburgh*, vol. xiii) offers him the hope of discovering upon these as many as four specimens of a curious parasite. In the inevitable 'preliminary notice' he introduces us to an unnamed, unfigured, dimorphic (but in the female sex almost shapeless) animal, which he has found parasitic on the vas deferens of *Nephrops norwegicus*. He believed it to be a Copepod of the family Lernæopodidæ. In the members of that group no peculiarity of situation, no oddity of form, can any longer excite surprise. What really surprises is that the Demonstrator

of Zoology in the New School of Medicine at Edinburgh should have been at the pains to prepare figures of his new species and yet publish his paper without them, that he should have evidence of its affinities which he is confessedly holding back, and that he should spoil the interest of his completed story by first forcing it upon the student in a rough unpolished sketch. It is satisfactory to learn that in both sexes of the new crustacean there is a 'niching on the dorsal surface,' though there is no such word as 'niching' in Webster's Dictionary. To explain the position of the parasite 'on the walls of the vas deferens' Mr. Thompson suggests the possibility 'that it entered the male reproductive duct as a larval form.' Whether the adults were found outside the duct or within it is nowhere explicitly stated.

THE HINGE IN BIVALVE MOLLUSCS.

WE last referred to Dr. F. Bernard's notes on the development and morphology of the hinges in bivalve Mollusca in our December number (vol. ix., p. 358); the third instalment has now appeared (*Bull. Soc. Géol. France*, Sér. iii., tom. xxiv., pp. 412–49: figs.). In it the Anisomyaria, or such shells as the mussel, scallop and oyster, and their allies, are dealt with. In all these the prodissoconch begins with a crenulated margin, as in *Nucula* and *Arca*, the true teeth forming later. Only in the Spondylidæ, however, do these attain any conspicuous development; whilst in *Ostrea* they never develop at all, the denticles sometimes visible in its adult shell corresponding to the crenulations in the embryo. In the Mytilidæ the secondary development of the ligament extends backward, and swamps all trace of posterior teeth, whilst the anterior ones are represented merely by irregular thickenings, which are scarcely, if at all, functional. *Dreissensia*, which has been placed now with the Mytilidæ, and now relegated elsewhere, shows no affinity with that group so far as the morphology of its hinge is concerned; but of this genus we are to hear more anon.

SPECIES MANUFACTURE.

WE are extremely glad to see that G. Dollfus, following in the wake of G. Coutagne, raises his voice (*Feuille Jeunes Naturalistes*, Nov., 1896), in protest against the wanton and pernicious multiplication of so-called species indulged in by certain of his malacological brethren, especially in the neighbourhood of Lyons.

The needed reform could only come from within, and we heartily welcome these signs of its advent. We gather further from the article that Mr. Coutagne proposes to bring out a genuine catalogue of the non-marine Mollusca of France, but we more than echo Mr. Dollfus' mild protest against Mr. Coutagne's apparent intention to disregard the rules of priority and to coin new names for his species. It is just this assertion of the "almighty ego" that has been at the bottom of all the mischief in nomenclature which it is his object in part to undo, and that, if adhered to, will deprive his catalogue of almost all its value.

571.14

I.

Worked Flints from the Cromer Forest Bed.

FOR the benefit of those not familiar with the geology of the Cromer district, I may, perhaps, be allowed to give the following particulars:—The valley to the west of Cromer, towards Sherringham, is still cloaked with a palæolithic gravel, which sometimes attains a thickness of eight or ten feet, and forms the summit of the cliffs. It is of light-brown colour generally, and implements of well-known palæolithic forms have been found in it for the last twenty years. They are well made, well finished, highly patinated, and only very lightly iron-stained, of an even uniform light-brown colour; calling strongly to mind the general facies of the Stanton Downham palæoliths. Flint flakes are also found in the gravel; and occasionally an implement gets washed out and falls upon the beach, where naturally it gets knocked about. But let the abrasive and destructive beach-action be what it may, the implements still retain their type and colour, and worked flints from this bed can readily be distinguished from those derived from the underlying deposits.

Below this gravel come the fantastically twisted and folded Glacial beds, appropriately termed the contorted drift, now sweeping in a low, long graceful wave, such as one would see in mid-ocean on the surface of the Atlantic, and now, amidst countless undulations of indescribable paths, trending almost vertically for from thirty to forty feet; then perhaps descending just as quickly, embracing a column of white or reddish brown material, which a lens reveals to be the old Crag or Cromer Forest Bed torn up in this fell swoop, push, or slide, and crushed to powder. In other places the contained fossils are less crushed, and frequently fragments are left of sufficient size for specific determination, especially when the torn-up mass is large, as some of them are; indeed, in the case of the great chalk islands, some of them are from two to three hundred yards long, and are now quarried for lime burning. But size and weight appear to have been nothing to the forces which operated during the formation of these remarkable deposits, as these huge masses of chalk were picked up, rolled over, and stuck on end, apparently as easily as the fragments of Forest Bed were carried up to the top of the cliff. Some amount of care is therefore necessary in reading the beds exposed in these cliffs. But all the abnormally violent action had ceased before the incoming of the comparatively peaceful conditions, under which the palæolithic

gravels were deposited, since their deposition has never disturbed the underlying beds. The glacial deposits, on the other hand, may contain anything older than themselves, down to and including the Chalk.

Below the Glacial beds is the Forest Bed series. Into a full description of this we need scarcely enter here, suffice to observe that every one now knows this is not a Forest Bed at all, but was deposited in the estuary of a river, which during its history—probably like many others—fluctuated in salinity. When we first meet it, it is fresh, then a N.E. subsidence asserts itself and estuarine conditions obtain, but only for a while, and the last we see of this interesting link with the Pliocene period is that its waters are running fresh. During many parts of its life, at least, it was a powerful stream, sometimes cutting through the underlying Weybourn Crag. The latter, when present, rests upon the Chalk. Both in the Weybourn Crag and the Forest Bed, when they rest upon an impermeable sub-stratum, there is often developed a very heavy iron pan. One of these occurs in the Forest Bed, and is distinguished from the rest by the number of large mammalian bones it has preserved, from which fact it has long been known as the Elephant Bed. There is another important feature about this bed which perhaps has not been publicly noticed, or at any rate I do not know that any one has drawn attention to the fact, although many have noticed it, and that is, its property of imparting to the contained fossils an amethystine tinge. This colour in silica is attributed to manganese, but as there is so much of this metal in most gravels without this coloration, it may be supposed that in the form of manganese dioxide, in which it occurs in ordinary river deposits, it does not so colour; but that as another oxide it may; and possibly the original dioxide of this pan may have been so altered, and thus colours not only the bones, but the contained flints. When the iron is the more active, we get the flints of a brown colour, with various amethystine tinges; when the reverse is the case, we get the flint assuming a beautiful black, with the same amethystine hue pervading. This staining process is often very deceptive; at first sight, a flint appears quite fresh, but the moment the edges are abraded the fact is revealed that the flint is very much altered. As might be expected, there is every intervening stage of alteration present, but all are characterised by this amethystine tinge. This peculiarity may at first appear unimportant, but when we remember that it is characteristic of the bed under consideration, it becomes of very great importance, as when present it enables one to fix the horizon of a flint or bone after it has been washed out of its original matrix.

To come at last to the main subject of this paper. Recently in working this "Elephant Bed" for bones, I was delighted, after breaking away some eighteen inches of the iron pan, to see sticking out what appeared to be an unmistakable worked flint. I called my wife to see it *in situ*, before it was removed, since I fully realised the opposition that would be raised by a certain section of

anthropologists to a Forest Bed implement. However, I left sufficient "pan" upon it to prove its original position.

By reference to Fig. 1, it will be seen that this flaked flint presents the outline of the hand-pick type of implement, a type that has come down to palæolithic times; many collectors, like myself, will be able to pair it from the Thames and other valleys. It is generally supposed that these were held in the hand, before the process of hafting had been discovered. There is a broad flaked face opposite the point, and the shape fits the hand in a most suggestive manner. It will be seen that whatever part "starchy fissure" or natural forces might have played in operating on the original flint nodule after it left the Chalk, there are only very few facets that do not show the well marked conchoidal ripplings, so characteristic of man's work; and practically whenever possible there are the characteristic pits of concussion, from which the bulbed flakes were removed when the tool was brought into its present outline; and in the last side operated upon these are quite parallel for more than three-fourths of its length, and even turn the corner in the same zone.

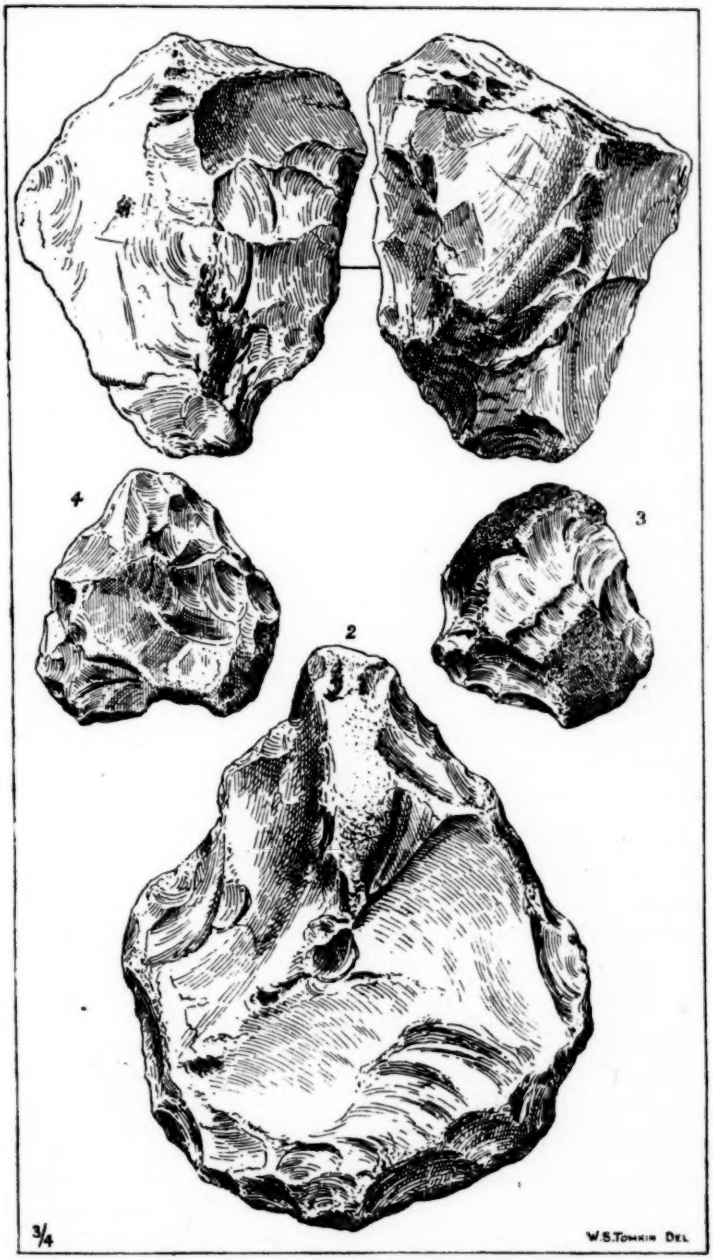
These are points of inestimable importance: often a flint has been made to simulate an implement in outline through the removal of pieces or flakes by natural agencies—colloid fissure, prismatic fissure, frost-fracture, vicissitudes of gravel-making, etc.—associated with other facets which, from their conchoidal fracture, we might attribute to man, and thus hastily give him credit for an implement he never made. In such specimens, the naturally removed flakes clearly post-date those supposed to have been removed by man, truncating or transversely bisecting the original flake-facet, removing all traces of the pit of concussion, which must have originally existed, perhaps several inches beyond the present periphery. It is certain that whatever may be the outline presented by such a flint, it manifests no marks of design, and cannot be regarded as the work of man. In an artificially-worked flint, the last flakes taken off which bring the implement into the desired shape should truncate or remove the pits of concussion from the earlier made facets, and carry with them their own pits as evidence of the part they played. I am fully aware that this is a most exacting test, and that by its application many a specimen, at present cherished, would be rejected; but it errs on the side of preventing us accepting anything as human, which may be the result of combined natural forces. Tested by these uncompromising rules, the specimens here described, from the Forest Bed, come out unscathed.

The point of the implement shown in Fig. 1, and the edges that would be exposed if used in the manner suggested, show signs of abrasion, such as would be produced in active service; and when we see that these edges are obtuse, and that of all the others—some of which are quite acute—none show any such marks, may we not fairly consider this the rational explanation of the facts presented by

this interesting specimen? I am fully aware of the fact that implements ought to be seen before one passes an opinion upon them; and I am equally conscious that before one can determine the possibilities of natural forces, and distinguish them from man's work, one should have years of actual and practical experience—not so much in rummaging amongst second-hand collections, nor even in visiting pits and buying implements from workmen, but in making them oneself. When one understands so much of the working of flint as to be able to set out and make an implement of any pattern and style of work desired, and also is able to tell a modern forgery at sight, then one is qualified to say what man or nature can or cannot do. In the eye of such a one as this, I am certain that the present specimen would improve upon acquaintance.

Fig. 3 shows a worked bulbed flake from the same bed as no. 1. It is one of the purple-black specimens. Only a very small portion of the original surface remains. The removed flakes in all cases were detached by sharp well-directed blows administered from the bulb-face, and frequently took a clean sharp direction quite across the flint, and when physically possible always show conchoidal ripplings and pits of concussion. It is this secondary flaking which gives rise to the present outline: only a very small portion of the original unworked surface of the flint remains. This specimen shows a well-marked *éraillure*, the meaning and importance of which must be explained at greater length.

I have tried some thousands of experiments of fixing flints and pitching round pebbles at them, and thus removing flakes, also by special suspension arrangements I am able to administer any number of blows at any particular spot with various degrees of force; but I have never yet been able to produce this scar in any way in which it may be conceived nature worked. In another set of experiments I have placed the flints, fixed and otherwise, at the bottom of a long inclined trough, and have let the stones slide down and strike the flint, or have put heavy weights behind them in a screw-jack action, both directly upon the flint and with the interposition of a loose pebble, but all with the same negative result. On the other hand, when I have tried to make a clean chop off a mineral scores of times I have been annoyed by a characteristic kick, giving rise to the *éraillure*. I explain it to myself in this manner:—In an ordinary blow one just brings the hammer upon the object, and is regardless of the rebound, which generally initiates the return motion, and thus is unrecognised; but when one wants to hit in a certain place, in a definite direction, there is an unconscious concentration of all muscular power to make the blow fall at that particular spot, and even keep the hammer there, and this voluntary muscular opposition offered to the uprise of the striker forces it back, occasioning a secondary but light blow. This can also be well seen and heard when one attempts to drive a nail in an awkward place by a series of slow



WORKED FLINTS FROM THE CROMER FOREST BED.

Three-fourths natural size.

deliberate blows, each of which will be followed by a second involuntary tap. It is this tap which removes the small flake from the bulb of percussion, and produces the well-known *éraillure*. This, therefore, is characteristic of an intentionally directed blow. Upon submitting specimen no. 3 to Mr. J. Allen Brown, F.G.S., he at once noticed this inestimable hallmark. All experiments thus point to the *éraillure* as being altogether more important than a mere bulb-of-percussion, and, so far as we are aware, may be taken as a proof of man's work, as it can easily be seen among flakes intentionally removed from a block, but so far as is known under no other circumstances.

Fig. 4 shows another example of the purple-black variety; but instead of being a chipped flake, it is worked all over, no portion of the original flint remaining unworked. The similarity to no. 3 in the flint, in the characteristic amethystine hue and in the work, is evident at a glance, and becomes more certain upon examination. The specimen was not obtained embedded in the pan, but at its outcrop on the foreshore below high-water mark, near the pan which yielded nos. 1 and 3, *Elephas meridionalis*, etc. No one who has seen the two and passed an opinion doubts for a moment that they both came from the same bed.

Fig. 2 depicts a very interesting implement. Like many of the very early plateau specimens, and the palæoliths, it is a naturally split flint and worked from one side only; the flaking is generally of a very bold and decisive character, although the edges show lighter but equally firm work, unaccompanied by contusion, thereby differing from the results of gravel-making, or beach-action, which invariably show the effect of undirected battering action. This implement was also out of the matrix when found, but its staining is so characteristic as to leave no doubt in my mind that it came out of the bed with which it was found associated.

I also discovered other specimens, some of which would doubtless strike some authorities as even more unmistakably human than those here figured, while the collection I was able to make of the stained and weathered flints from this bed would enable one to feel little doubt in deciding whether or no nos. 4 and 2 came from the Forest Bed. Although I have here referred only to the pan, I may say that I obtained a number of small bulbed and bulbed-and-pitted flakes from another part of the bed, and I am certain that patient and diligent research in these beds would be rewarded with specimens that would satisfy the greatest sceptic procurable. The above were all found to the west of East Runton. Mr. Clement Reid, F.G.S., writes me:—"I have always considered that if implements were found in the Forest Bed, it would be at Runton, although up to the present I have been unable to find any."

In conclusion, I may say that I have submitted these worked flints to a number of the first experts of the day, who have accorded them unqualified acceptance as being man's work. Sir John

Evans, F.R.S., who, as everybody knows, exercises with regard to such novelties a scientific caution that some might call extreme, and who has sometimes called himself the St. Thomas of anthropologists, writes:—"No. 4 may or may not be artificial, and the same may be said of no. 3, with even more probability of its being made by man."

Bearing past history in mind, and the reception which has been accorded to these specimens, the unquestionable evidences they present of being artificially worked, the unmistakable positions from which they were obtained, and the conditions under which some of them were found in the matrix, are we not justified in admitting the existence of man in Britain in the Forest Bed period?

W. J. LEWIS ABBOTT.

Sevenoaks, Kent.

595.2

II.

Are the Arthropoda a Natural Group?

IT was Von Siebold who, in 1845, formed the Insecta of Linnæus, or the Condylopoda of Latreille, into a primary division of the Animal Kingdom, which he called Arthropoda. For thirty years this was considered a natural group with close relationship between the four classes which composed it, and in 1869 Fritz Müller said that there could scarcely be a doubt that all these classes were branches of a common stem. The pedigree was traced as follows. The primitive thysanuriform insect and the primitive myriopod were supposed to have had a common hexapod terrestrial ancestor which was, probably, a descendant of an aquatic zoëa-form crustacean. The Crustacea and the Arachnida were traced to a primitive phyllopod, which appears to have descended from an unsegmented nauplius.

After the publication of Professor Moseley's papers on *Peripatus capensis*, the Malacopoda (De Blainville), or the Onychophora (Grube) were taken out of the Vermes and admitted, by most zoologists, as a fifth class of the Arthropoda. Professor Huxley, in his paper on the Classification of Animals (1876), says that *Peripatus* is a low and primitive arthropod, and thus affords evidence of the highest significance as to the relations of the Annelida with the Arthropoda. But if *Peripatus* is a primitive arthropod, it can only be a primitive tracheate arthropod and not a primitive branchiate arthropod. To find the common stock of *Peripatus* and the Crustacea, we must go back to the earliest forms of worms; and the characters which were formerly supposed to shew a close relationship between the Crustacea and the Insecta—including hollow-jointed limbs, exuviation of the skin, and the structure of the compound eyes—cannot be due to inheritance, but must have been evolved independently by what is called convergence of characters.

If this is correct, the Arthropoda must either be divided into two primary divisions of the Animal Kingdom, each equal to the Mollusca; or they must be united with the worms in Macleay's primary division of Annulosa; for I suppose it will be allowed that, in a natural classification, a group should include the common ancestors of each member. If, however, the older conception of the origin of insects be correct, then *Peripatus* cannot belong to the Arthropoda—for certainly it is not a retrograde myriopod—but must be looked upon as a polychæte worm specially modified for a terrestrial existence.

I offer no opinion myself on the subject, but should like to know how others view the situation.

Christchurch, N.Z. F. W. HUTTON.

No zoologist can be astonished that the genealogical trees of the animal kingdom, which have sprung up so plentifully during the last few decades, should require serious pruning. I take it therefore that Mr. Hutton's object is to ascertain how those who have made a special study of Arthropoda propose to arrange their genealogies at the present moment. Experience shows that the less one knows about a group, the easier is it to dash off its family tree, while, conversely, the more one knows, the more reluctant are the branches to group themselves in any satisfactory manner. Thus the genealogy of the Arthropoda will not be finally established until each of its component groups has received far more attention than has yet been devoted to it—that is, with this special object in view.

In giving my own suggested ancestry of the Arthropoda, I should state at the outset, that it is based upon special studies of only two of the groups concerned, the Crustacea and the Arachnida. The detailed arguments on which the following summaries of my conclusions are based, will be found in my "Comparative Morphology of the Galeodidæ" (*Trans. Linn. Soc., London*, 1896.) and in the other papers there cited, and further, in a summary now in the press for the March number of *Science Progress*.

(1). There is no difference of opinion in referring all the Arthropoda back to Chaetopod Annelids, but inasmuch as there is no reason to believe that the Arthropoda are the only derivatives from Chaetopoda, I should not count these latter also as Arthropoda. They are only the soil out of which the Arthropoda developed along a definite line, viz: the specialisation and structural modification of the parapodia for the purposes of prehension and locomotion.

(2). The lines along which these specialisations and modifications of the parapodia have developed for these purposes are, so far as my researches go, not the same in any two groups of Arthropoda, hence each group has to be deduced separately. The Arthropoda thus cannot be arranged in any single genealogical tree.

(3). The Crustacea. Regarding *Apus* and the Trilobita as the earliest and most annelidan Crustacea, we find that their peculiar specialisation was primarily the bending round of the prostomium ventrally to form a great labrum under which a certain number of parapodia could function as jaws within and around the mouth-aperture, one or more of the anterior pairs being left free to function as sensory organs. The parapodia posterior to the mouth-organs seem ventrally to have repeated the chewing functions of the jaws, while dorsally they developed into complicated swimming plates, retaining the original parapodial gill. The development of walking legs was secondary as is shown clearly by the Trilobita, which

retained the *Apus*-like swimming plates under the pygidium. The Crustacea are the only well established group which became arthropods while retaining an aquatic life.

(4). The Arachnida. The common ancestor of the arachnids was a chaetopod in which the two first pairs of parapodia were thrown forward, two above and two below the mouth, the first pair as seizing limbs, the second pair primarily as sensory, and only secondarily as seizing organs; the prostomium and labium were squeezed between these to form a beak for sucking the blood of victims crushed out by the above-mentioned limbs. This liquid food-supply, pumped in with force by the œsophagus, as can be shown in detail, modified the whole of the rest of the organisation. The locomotion necessary for catching living prey was highly specialised, four pairs of parapodia behind the two grouped round the mouth developing into long legs, while the posterior region became a great distensible sac with its limbs aborted.

(5). The Insecta. The primitive insect I believe to have been worm-like, with a first pair of parapodia developed into antennæ, and the three following pairs forced forward as jaws to work in an anterior mouth-aperture; the following pairs were originally but little developed for locomotion, being used rather for clinging to the leaves and stalks on which the insect fed. The herbivorous caterpillar represents, I believe, a true stage, and probably one of the earliest stages, in the development of the Insecta from their chaetopod ancestors.

(6). The Myriopoda. These have two distinct types of mouth-organs; if these two different modifications of parapodia cannot be deduced the one from the other, then I should be compelled to regard the Chilopoda and Diplopoda as two distinct derivations from Chaetopoda. There would surely be nothing astonishing in the fact that two (or, including *Peripatus*, three) different descendants of the Chaetopoda, on taking to a land life, retained the whole series of parapodia, *i.e.*, behind the mouth-organs, for locomotion.

(7). *Peripatus*. I regard this as a feebly developed arthropodan derivative of the Chaetopoda, an arthropod in making, and rightly classed with the rest though in no true sense an ancestral form of any of the foregoing.

(8). The Pycnogonida. Whether these are to be deduced from any of the above or are again distinct specialisations, I feel unable even to guess; they remain the most enigmatical of the Arthropoda.

Two further remarks on these brief notes on the phylogeny of the Arthropoda:—

(9). I have endeavoured elsewhere to show in detail how the distribution not only of glands but also of the tracheæ of the different arthropods may be fully and simply explained by deducing them all (? the coxal glands) from the bristle-sacs of the original chaetopodan parapodia.

(10). All these new forms of life arising out of that highly plastic form, the Chaetopod Annelid, were, I believe, due not to the selection of small chance congenital variations but to definite responses to the environment. In each case, the adoption of a new method of feeding gave rise, slowly but surely, to a new organisation.[1] All subsequent resemblances in the descendants of these ancestral forms are purely convergent.

HENRY M. BERNARD.

Babington Road, Streatham.

IT seems to me that the striking common structural characters of the classes of animals generally comprised in the Arthropoda oblige us to retain that phylum, at least until phylogenetic speculations regarding its dual origin stand on more certain ground. A primary division of the arthropods into branchiate and tracheate forms seems inadmissible, when we can trace the relationship between the water-breathing king-crab and the mites or harvestmen breathing by means of air-tubes. The morphological and embryological evidence, at present available, appears to point to many-limbed animals with a number of similar segments as the primitive arthropods—the ancestors of crustaceans, arachnids, diplopods, chilopods, and insects. It is likely enough that the stock which gave rise to the three last-named classes had adopted terrestrial life at a very early period. But this stock must have been of a more decidedly arthropodous character than our living *Peripatus*, which represents an offshoot at a much lower point in the evolution of the phylum. The superficial resemblance of *Peripatus* to a myriopod cannot blind us, for instance, to the fact that its antennæ are pre-oral, while those of insects and chilopods are primitively post-oral. And Mr. Sedgwick has lately stated his opinion that it stands about half-way between arthropods and annelids. If we recognise its excessively primitive nature, it may be most convenient to retain it in the arthropod phylum, remembering that its tracheæ—a necessary adaptation to a terrestrial life—do not require us to regard it as approximate to the immediate ancestors of insects and centipedes.

Dublin. GEO. H. CARPENTER.

May I point out that, first and foremost I am as little inclined to agree with the view put forward by F. Müller, and adopted by so many authors, that the nauplius represents the starting point and ancestral form of the Crustacea, as I am to agree with Haeckel's strongly formulated interpretation of the importance of the zoëa in the derivation of the Malacostraca. Twenty years ago in my "Untersuchungen zur Erforschung der Genealogischen Grundlage der Crustaceensystems" I sought to show that the zoëa, so extremely variable in form, has no phylogenetic significance, but only represents

[1] See *Nature*, vol. l., p. 546, 1894.

a larval form, secondarily arisen, into the organisation of which many later acquired characters have been inserted so as to alter it in various ways.

To apply this view to the explanation of the nauplius was a natural consequence. A few years later I myself came to this conclusion, having been caught and held fast by the alluring teaching of F. Müller, which Haeckel had also enthusiastically taken up; but when Hatschek had drawn, from an unbiassed study of the development of the annelids, his conclusion that the nauplius represented a changed trochophore larva, I also tried to point out in my paper "Neue Beiträge Zur Morphologie dei Crustaceen" (*Arbeiten der Zool. Inst. Wien*, 1886), that the nauplius must be looked upon as an altered larval form of the annelid stock with accelerated arthropod characters. "If we cannot distinguish the genetic relations between arthropods and annelids in respect of metamerism of the body, of the similar processes of separation of the segments at the hinder section of the body of a growing animal, and of the general agreement in internal structure, especially that of the brain and ganglion chain, of the segmental organs (*Peripatus*), shell glands, antennary glands, limbs and parapodia, then we have to imagine as the ancestral form of the Protostraca (Archiphyllopoda) an organism like a many-limbed annelid with incipient arthropod characters, especially in the structure of the extremities" (*l.c.* pp. 93, 94).

If we view the nauplius form from such a standpoint, the dilemma which Hutton insists upon with regard to the origin of the Onychophora falls to the ground, and there is no reason to disallow the probability of all classes of Arthropoda having descended from annelids. No one can deny that the Onychophora represent the class nearest related to the annelids. They will therefore correspond to a branch which became cut off from the common stem quite early, even before any important reduction in the segmental organs took place, and acquired in connection with a land-habitat their peculiar tracheal respiration, differing from that of the insects and myriopods. From this stem the ancestral form of the hypothetical Protostraca or Archiphyllopoda may also have arisen. In fact, we have to think of it as having had some such form as I tried to construct at that time ("Crust. Syst.," *l.c.* p. 100). Arnold Lang, in his excellent text-book of Comparative Anatomy (1891), has drawn a more complete picture of this supposed stem-form, which corresponds to advances in knowledge since the time at which I wrote. I can express myself as agreeing in all essential points with this scheme, as regards both the internal structure and external form.

That the Myriopoda and Insecta which are nearly related to one another, and therefore are sometimes put together in one arthropod class, as Antennata, have the same origin as the Onychophora, appears to me as little admissible as the erroneous view which derives them from an aquatic zoëa. More rightly should the Antennata be

looked upon as a third group of arthropods, derived from a special branch of the chief stem.

Similarly the Arachnoidea must hold the position of a fourth arthropod group. As is well known, different views have been taken as to the relations of the Arachnoidea. According to those authors who make a fundamental difference between gill- and tracheal respiration, they form, in connection with the insects, myriopods, and Onychophora, a sub-phylum of the Arthropoda, viz., Tracheata. The second sub-phylum is that of the Branchiata, which includes, besides the Crustacea, the Gigantostraca, Trilobita, and Xiphosura (Limulus). On the other hand, a second school of authors holds the Arachnoidea to be related to the three last-named groups, and classifies them together under the head Palæostraca. This conception, which I hold to be the right one, depends on the remarkable agreement shown by the segmentation of the body and morphological structure of the Palæostraca and the number and position of their limbs with what obtains in the scorpions; and it is furthermore strengthened by the similarities observed between the internal structure of *Limulus* and *Scorpio* (endoskeleton, coxal glands, structure and position of gill- and lung-books). The highly organised Arachnoidea would therefore stand nearest to the oldest and most original forms of this group of arthropods, and have developed from water-living Branchiata into terrestrial Tracheata. As regards the transformation of the gill-plates carried in chambers on the ventral surface into the lamellæ of the so-called lungs or tracheal chambers, it seems to me that McLeod has given a likely explanation.

But now the question comes,—are we to consider this fourth arthropod group as an independent branch of the arthropod stem, or are we to derive it from the Protostraca? The latter view would commend itself if we could succeed in discovering a nauplius-like larval form in the young stages of the Palæostraca. Of course the early stages of *Limulus* have been compared to trilobites, and on the other hand the oldest and most primitive trilobite (*Protaspis*), the larval nature of which cannot well be doubted, has been described as the forerunner of the crustacean nauplius and called Protonauplius (Beecher). Nevertheless, satisfactory proofs of this very interesting point are wanting, especially the presence of limbs, the structure and position of which are not known. So long as the necessary proofs are in abeyance, we must attach greater probability to the first of our suppositions, and thereby look upon the Arachnoidea and their predecessors the Palæostraca as a group arisen independently and having no connection with the Crustacea.

It is not necessary for me to emphasize the fact that in my discussion I have not dealt with proved facts, but only with views which claim a right to be heard, in deciding the relationship of the classes of Arthropoda from a standpoint based on the theory of descent.

Vienna. C. CLAUS.

I THANK you for your letter with the enclosed communication by Professor Hutton. But it is impossible for me, in only a few days, to work out so good an answer as I could wish, besides, for the present I am much engaged with other work.

During more than ten years I have spent much time preparing a paper on the structure and morphology of the skeleton, etc., in all orders of Crustacea and Myriopoda, and some orders of Insecta. A kind of preliminary note has been published in the *Zoologischer Anzeiger* for 1893 (translated in *Ann. & Mag. Nat. Hist.*, 6 ser., vol. xii., pp. 417–34), and I hope that the final paper will be published in a few years. Before this publication I do not wish to enter into any further discussion on this subject, as I believe it will be better to be able to refer readers to the full representation of the numerous details in the said work.

In this short answer I cannot make use of the modern terminology, since I dislike ancestor-hunting and pedigrees. It seems to me that too many authors take special pleasure in the construction of new "trees," and I must confess that I am not able to recognise the

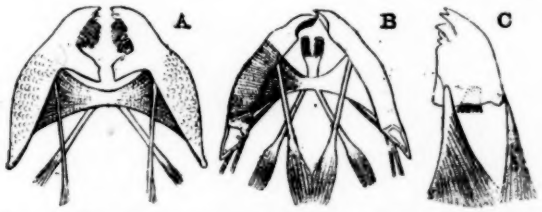


FIG. 1.—MANDIBLES WITH THEIR MUSCLES SEEN FROM BELOW.

A. *Diastylis goodsiri*, Bell. B. *Japyx solifugus*, Hal. (after Meinert, 1865). C. *Blatta* (*Periplaneta*) *Americana*, Fabr.

utility of these interesting forests. As to the Arthropoda, new facts, thorough-going studies of the adult animals are exceedingly wanted; further speculations on the facts at present generally known will, in my opinion, be of no value.

And now some remarks on the question itself. The lower malacostracous Crustacea and the Thysanura are, in my opinion, more closely related to each other than hitherto recognised. Here I shall produce but some few facts. If good figures of the mandibles with their muscles of *Nebalia*, *Diastylis* (belonging to the Cumacea) and *Blatta*, as types for lower Malacostraca and for the Orthoptera, were laid before a zoologist, and then figures of the same parts of two Thysanura, such as *Machilis* and *Japyx*, were shown him without telling him the names of these two animals, he would say without hesitation that the mandibles of the Thysanura belonged to Crustacea, and not at all to Insecta. The three accompanying figures (Fig. 1) will, I hope, illustrate this statement, and the following explanation will be satisfactory for this purpose. In *Diastylis* and *Japyx* we find

a common median, free, internal plate with enormous muscles radiating into the elongated lateral cavities of the mandibles; and the two other pairs of muscles, with the long tendons, also are very characteristic (two small pairs of muscles issuing from the superior margin of the mandibles are not visible). In *Blatta* the cavity of the mandible turns backward, no median free plate exists, and of muscles we find three: an enormous *musculus adductor* from the inner edge, a *musculus abductor* from the outer edge, and a short oblique muscle from the interior wall of the mandible. It may be remarked that *Blatta*, which has been investigated by numerous authors, is without good reason generally considered as being of low organisation; the distance from *Blatta* to such low forms as *Japyx* and *Machilis* is, in reality, rather great, at least as great as between *Blatta* and Coleoptera.

Lubbock makes mention of a second pair of maxillæ in the Thysanuran family Collembola; the limbs he has seen are the *first* pair of maxillæ, as they originate in advance of the second well-known pair on both sides of hypopharynx; in *Japyx* this first pair possesses a well-developed, three-jointed palp. Thus, in the Thysanura we observe four pairs of mouth-limbs, homologous with the four pairs in Isopoda and Amphipoda; the conformity exists to such a degree that in *Machilis* and the Malacostraca the two lobes on the second maxilla originate from the *second* and the third joints, while the lobes on the first maxilla in the lower Malacostraca are produced from the *first* and the third joints; finally, in the Amphipoda the fourth pair, the maxillipeds, has one or two of the joints coalesced in the median line, thus forming a kind of lower lip with palps—as the fourth pair, the labium, in *Machilis*, etc. (These and numerous other facts are mentioned in my preliminary note of 1893.)

Not very far distant from the Thysanura we find *Scolopendrella*, which forms a transition to the Chilopoda and the Diplopoda, the distance between these three orders being much shorter than that between, say, Phyllopoda, Isopoda, and Copepoda.

The Crustacea, the Insecta, and the Myriopoda, are, in my opinion, more closely related to each other than are the Arachnida to any of them—not to mention the Pantopoda, with good reason now generally accepted as a separate class.

It is now, I think, generally admitted that the larval stages: nauplius, zoëa, larvæ of insects, etc., have been very much over-estimated in judging the relationships between the different classes of Arthropoda and the relation of the whole series to lower animals. In Korschelt and Heider's well-known "Lehrbuch" (vol. i., p. 904), we find a similar view.

The well-known characters for the four old Classes of Arthropoda: the jointed legs (in all forms not much degraded by parasitism), the arrangement and the quality of the muscles, the exuviation of the skin, the eyes, etc., are, in my opinion, good evidence of their rather close relationship to each other.

Peripatus I have never examined, but, so far as I can see, it stands apart from all Arthropoda and must be removed from the series. Korschelt and Heider have placed it among these animals, in front of the Myriopoda, and have put together all the essential characters known in 1890; but this same chapter of their work shows very plainly the enormous differences between *Peripatus* and the five classes of Arthropoda, with the structure of which I am tolerably acquainted through studies during many years. Such features as the quality and arrangement of muscles, the series of nephridia, the structure of the mouth and the "limbs," the eyes, and the generative organs with their ducts seem to be sufficient. Furthermore, it will be admitted that *Peripatus* has nothing to do with the Crustacea, and since *Scolopendrella* and the Thysanura show a closer relation to the lower Malacostraca than hitherto recognised, while the Myriopoda are divergent, specialised forms (for instance, the "gnathochilarium" is, in spite of embryology, formed by the coalescence of two pairs of mouth-limbs), I think that no place for *Peripatus* will be found among the Arthropoda. I am most inclined to look upon this animal as a "worm specially modified for a terrestrial existence," and it must, in all probability, take the rank of a separate class between the multitude of very different forms together composing the "worms" (such as Annulata, Chaetognatha, Sipunculida, Nemertina, etc.) whose mutual relationships are still in part rather obscure.

H. J. Hansen.

Copenhagen.

The conclusion of F. W. Hutton as to the position of *Peripatus*, expresses the view which I also hold, and have expressed in *Jenaische Zeitschrift*, Bd. xxx., p. 67, 1896.

In a treatise entitled "Die Entwickelung der sog. Lungen bei den Arachniden, &c.," (*Zeitschr. wiss. Zool.* Bd. lviii.: See also *Muzeum Lemberg*, 1896), in connection with the discovery that, in *Trochosa*, the so-called lung passes into tracheal branches, while in other spiders and in scorpions it is sac-like and thus must actually be regarded as a sac-trachea which does not develop further, I have shown that the view that the Arachnida are to be derived from Crustacea, whose respiratory appendages sank beneath the surface of the body and thus led to the formation of arachnidan lungs, cannot be maintained. On the contrary, anatomy and ontogeny both show that the arthropodan appendages, together with the gills, are to be deduced from the lungs.

It is self-evident that the Arthropoda are descended from those lower worm-like animals which, in adaptation to terrestrial life, developed integumental depressions, *i.e.*, sac-tracheæ, for the purpose of breathing air; the possibility that this adaptation was of special utility to animals dwelling on the shore and subject to the recurrence of the tides is by no means denied. Exposure to air and sun brought

about, for protection, the hardening of the integument, while locomotion gave rise to annulation. In this condition of the body, the animal could subsist in a damp, slimy medium. It is justifiable to assume that limbs were developed in adaptation to life on dry land, where the skin would suffer from friction and the respiratory organs from exposure to dust. Simple lateral sac-tracheæ (Fig. 2 A*a*) carried on respiration also during the rainy season: more highly developed tracheæ (lungs) with folded walls (Fig. 2 A*b*), especially when muscles were associated with them, afforded the animal the means of closing the sacs, concomitantly with the development of the first rudiments of the locomotory organs by the evagination of the folds (Fig. 2 B).

Ontogeny teaches us that the limbs, as a rule, do not begin to form simultaneously along the trunk, but develop from before backward. The anterior portion of the body thus became adapted for life on land sooner than the posterior portion: it is therefore not surprising that, as stronger subsequently developed musculature

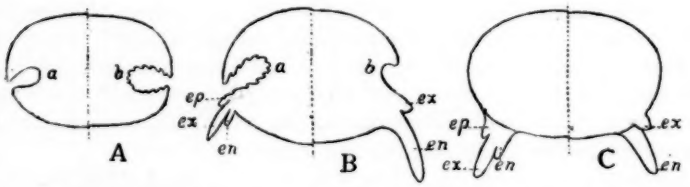


FIG. 2.—COMPARATIVE DIAGRAMS ILLUSTRATING EVOLUTION OF LUNGS AND LIMBS.

A—Combined diagram. *a*, simple sac-trachea (lung); *b*, sac-trachea provided with folds.

B—Combined diagram. *a*, a folded sac-trachea; *b*, the remains of the so-called lung; *e*, epipodite; *ex*, exopodite; *en*, endopodite.

C—Combined diagram. *ep*, epipodite; *ex*, exopodite; *en*, endopodite. (In *Trochosa singoriensis*, Laxm.)

appeared at the anterior part of the body, the respiratory organs vanished, but left behind the limbs as vestiges, while, in the posterior part, the respiratory organs persisted in a high state of development. Whereas, if the whole body had become simultaneously adapted to land life, or else one part shortly after the other, it would have retained the original worm-type (Myriopoda).

But how have the Crustacea arisen? Their structure, considered anatomically and ontogenetically, leaves no doubt of their racial relationship to the other Arthropoda. The annulation of the exoskeleton and the development of the limbs must, in them, have been determined by the same causes as in the other Arthropoda. The Crustacea, as Simroth has so conclusively argued in his "Entstehung der Landthiere," 1891, were originally terrestrial, their limbs serving for locomotion on land. They may have been compelled by the search for food, to take to an aquatic life. The exo-, endo-, and epi-podite developed from lung lamellæ for increase of the

respiratory surface. The gills are the homologues of the lung lamellæ, since it has been established by Kingsley's interesting researches (*Quart. Journ. Micr. Sci.*, 1885) that the limbs of *Limulus* are protruded from a depression; and in *Ligia*, *Oniscus*, and other Crustacea,[1] similar invaginations have been observed near the limbs. In all groups of Arthropoda, indeed, in the embryo, limbs are to be found which reveal the primitive type[2] (Fig. 2 C). Since, therefore, the Crustacea became aquatic, it is evident that the depressions (sac-tracheæ) functioning as respiratory organs must not only have ceased to develop further, but must have degenerated, and the absence of tracheæ in this class is thus accounted for.

The parapodia and gills of the annelids are due to the same evolutionary factors. The annulation of the annelid body and the hardening of the integument at any rate testify to the fact that they also are of terrestrial origin. The original sac-like respiratory organs have disappeared, the parapodia and the branchiæ remaining behind as vestiges.

While referring the reader for details to the original works on the subject, I think that in thus deducing arthropod limbs from lungs I have disposed of the assumed derivation of the Tracheata from the Branchiata, and have come nearer to the true origin of the Arthropoda, more especially of *Peripatus*.

It is, however, difficult to arrive at definite conclusions as to the systematic position of the last-named arthropod. We are as yet only on the threshold of a great building in one corner of which truth dwells. The assumption that *Peripatus* is most nearly related to the most highly developed annelid, and Hutton's conclusion that the former should be directly classed with the latter are not without justification. It is certain that *Peripatus* cannot possibly be derived from the Crustacea. I incline to the view that *Peripatus* is a secondarily adapted land animal. In the first process of adaptation, it acquired gills and parapodia-like truncated limbs, which, owing to aquatic life, could not develop further. The animal, in this process, lost its respiratory organs. Later, by gradual *re-adaptation*, it re-acquired breathing organs in the form of trachea-like invaginations of the integument, sometimes scattered all over the body. This view gains in probability from the fact that other arthropods (*Oniscus*, *Birgus latro*) have become readapted to land life.

When we consider the origin of the limbs and the gills, as well as of that of the parapodia and gills of the annelids, we arrive at the conclusion that water and land have from the first shaped these animals, and that they are all to be derived from worm-like ancestors. In this case, *Peripatus* represents a transition form between the Arthropoda and the highest annelids, being connected with the former

[1] cf. *Zeitschr. fur wiss. Zool.*, Bd. lviii., pp. 54–78; *Muzeum Lemberg*, 1896; *Zool. Anz.*, Nos. 455 and 473.

[2] cf. also *Zool. Anz.* Nos. 363 and 364, and especially No. 392, where a case is cited of a limb possessing an appendage as well as the endo-, exo- and epi-podite.

by the structure of the limbs, especially of the jaws, and of the tracheæ, and with the latter by the slight development of the limbs. It was merely a freak of nature, that the conditions which influenced the formation of the body of *Peripatus* were different from those that affected other arthropods. After careful consideration of the comparative anatomy and ontogeny of these animals, the type "Arthropod" appears to me, if we omit their primitive ancestors, not sufficiently comprehensive, and I recognise the advisability of accepting Cuvier's type Articulata, divided into Arthropoda and Annelida, all the more since this type could, by means of the Trochophore, be united with the Mollusca in a common genealogical tree.

A. JAWOROWSKI.[1]

Lemberg.

THE questions raised by Captain Hutton should have more space for their adequate discussion than NATURAL SCIENCE can afford. I will therefore give my views upon the subject in outline only, and would refer the reader for more details and for a tolerable bibliography to my paper on the Classification of the Arthropoda (*American Naturalist*, vol. xxviii., 1894, Tuft's College Studies, No. 1). Some thirteen years ago (*Amer. Nat.*, vol. xvii., 1883) I was troubled with almost the same questions as those bothering my antipodean colleague, and now, although my line of argument would be much different from that then adopted, my views are essentially unchanged. It is interesting to note that von Kennel, in his "Lehrbuch der Zoologie" (1893) and in other papers, has adopted similar views. His researches upon *Peripatus* entitle his opinions to consideration.

The first question, that in regard to the importance of the nauplius, can be briefly answered. I think that most morphologists at present regard it as an adaptive condition introduced early in the phylum and not as representing an ancestral condition. Any other view leads to inextricable difficulties. The nauplius, I think, can safely be ignored in any discussion of the inter-relationships of the Arthropoda.

Next comes the question concerning *Peripatus*. Has it that importance in phylogeny which it seemed it must have when Moseley's investigations were first published? Is it an arthropod? Is it an annelid? Or is it a representative of a distinct group? As my views upon these points are somewhat at variance with those held by many morphologists, I may be permitted to say a few words concerning them.

Peripatus differs from all arthropods in several features. It has functional nephridia, repeated metamerically in most of the segments of the body; while so far as I am aware no true arthropod has more than two of these organs. Striped muscle is found nowhere in

[1] Kindly translated from the German by Mrs. H. M. Bernard.

Peripatus, except in connection with the jaws. The antennæ are outgrowths from the primitively pre-oral region, and hence cannot be compared with the antennæ of Insecta or either pair of antennæ in Crustacea. The ciliation of the nephridia and alimentary canal is without parallel in the Arthropoda. The external cuticular skeleton is weakly developed and the legs show no true jointing. Then, too, the eyes are unlike those of any true arthropod, but are strikingly like those of the polychæte *Autolytus*, Fig. 3. It is further to be remarked that in *Autolytus* there are two pairs of eyes, the second pair occupying the position of the problematical structures labelled *d* by Balfour (*Quart. Journ. Micr. Sci.*, xxiii. pl. xviii., fig. 22). The muscular nature of the pharynx is not arthropodan but resembles rather that of the Chætopoda.

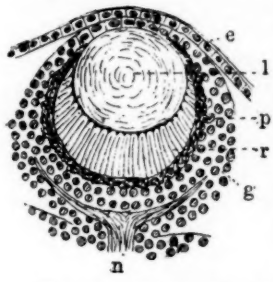


Fig. 3. Section of eye of Annelid, *Autolytus*. *e*, epidermis; *g*, ganglion layer; *l*, lens; *n*, optic nerve; *p*, pigment layer; *r*, rods. Compare with eye of *Peripatus* as figured by Balfour, *Quart. Journ. Micr. Sci.*, xxiii., pl. xviii., fig. 22, 1883.

On the other hand the features relied upon for associating *Peripatus* with the (tracheate) Arthropoda are the following:—the presence of tracheæ; but we must remember that at least two other kinds of tracheæ occur in the Arthropoda which could in no way be derived from those of *Peripatus*, and further, that the resemblances between the tracheæ of *Peripatus* and those of Hexapoda are, aside from the physiological one, extremely slight. In fact the evidence, so far as tracheæ are concerned, points to convergence rather than to community of descent. In *Peripatus* a pair of nephridia is set apart exclusively for reproduction purposes, a condition which recalls the structures in both Hexapoda and Crustacea; but we must not forget that similar relations occur among certain annelids as well as in other groups. Of more importance are the facts that the dorsal vessel, with its several ostia, is enclosed in a pericardial chamber, while the circulation is lacunar and the cœlom is much more reduced than in most annelids. To these might be added the claws terminating the legs and the character of the jaws, apparently appendicular in nature. We must, however, remember that so far as our knowledge goes, we are doing no great violence to morphology when we compare the feet of *Peripatus* with the appendages of a syllid worm, or when we compare their terminal claws to chætopod setæ. In short, I may say that there is a pretty large amount of doubt as to whether the Malacopoda are Arthropoda.

Now, leaving *Peripatus* and the nauplius out of the question, we are met with the greater problem:—Is the group Arthropoda a valid one? Or is it rather an assemblage of convergent forms with no common ancestor nearer than the annelids?

Omitting all considerations of minor groups like the Tardigrada, Linguatulida and Pycnogonida, I think that recent students have shown that there are three large groups among the forms commonly considered as Arthropoda. The first of these, the Branchiata, includes the Crustacea and Arachnida. The arguments for this association have been so often presented that I need not rehearse them here. For the second group the name Insecta or Antennata may be employed. It includes the Hexapoda and the Chilopoda. Both Pocock and myself have repeatedly pointed out that there is no valid group of Myriopoda. The Chilopoda are decidedly like the Hexapoda in their broader anatomical features, while the Diplopoda[1] differ so totally from these that they may form our third group. Concerning these Diplopoda, however, so little is known that they may be left out of consideration in what follows.

For convenience of contrast of the characters of the Branchiata and Antennata I employ parallel columns.

BRANCHIATA.	ANTENNATA.
Respiration by gills (or lungs or tracheæ derived from gills) arising in connection with appendages.	Respiration by tracheæ (modified glands ?) never connected in origin with the appendages.
Functional nephridia in somites, two, five, or both.	No functional nephridia.
Genital ducts (modified nephridia) opening near the middle of the body.	Genital ducts (modified nephridia) opening near the end of the body.
Malpighian tubes (when present) entodermal.	Malpighian tubes ectodermal.
Stomodaeum long.	Stomodaeum short.
"Head" indefinite, ranging great y in limits even in the same group.	Head distinct, consisting of pre-cephalic lobes and five segments (as indicated by the neuromeres).

These are, it seems to me, points of fundamental importance, and I can hardly see how one can reconcile them without going back to the annelid ancestor. Certain it is that the two groups must have become differentiated at a time when nephridia were present in all segments, when these served as the only excretory organs, when there was no head comprised of coalesced somites, when there were no specialised respiratory organs apart from the general body-surface or possibly portions of the appendages, and when no appendages were specialised as jaws to the exclusion of others. No known form outside the annelids meets these requirements; and these facts, it seems to me, go far towards showing that the group Arthropoda does not form a true phylum, and that the similarities which are often advanced to support such a view are the results of convergence, many of them explicable as homoplastic.

J. S. KINGSLEY.

Tufts College, Mass., U.S.A.

[1] F. C. Kenyon has recently shown that *Pauropus* is an aberrant dipiopod: "The Morphology and Classification of the Pauropoda," *Tufts College Studies*, No. 4, 1895.

MR. F. W. HUTTON'S letter opens a question which has engaged my attention for some time, and I think that his fears for the Arthropoda are fully justified by the facts. I have long felt that, while the term Arthropoda is a very convenient one as expressing a certain kind of structural modification, it does not imply a phylogenetic relationship among the forms included in it. In short, the Arthropoda had not an arthropod common ancestor, and jointed legs cannot be regarded as an absolute criterion of relationship.

The position of *Peripatus* is closely associated with this question. Its claims to be included among the Arthropoda are too well known for it to be necessary to recapitulate them here, though we shall see that one of them—the presence of tracheæ—is of doubtful value. If we admit *Peripatus* as a primitive arthropod—an offshoot not far removed from the ancestral stem—the ancestral stem can only be that of the Myriopoda and Insecta. It is quite impossible to insert *Peripatus* into the ancestry of the Crustacea, and it is equally impossible to find a place for it in any line passing from the Crustacea to the Insecta. The Arachnida must also be excluded from this series, as any attempt to derive the Arachnida from a tracheate ancestry leads only to confusion worse confounded. Even if we leave the probable ancestry of the terrestrial arachnids from aquatic forms, related to *Limulus* and the eurypterids, out of the question, we find that the lower and more primitive members of the class are in every case provided with a respiratory apparatus of peculiar and characteristic form, which must, in this case, be secondary and developed from tracheæ. This being the case not only with the lower orders, such as Scorpionidea and Pedipalpi, but the lower members of the Araneina having also no tracheæ, any attempt to construct a phylogenetic tree starting from tracheate forms ends in failure. We must therefore admit that tracheæ have been developed twice along two independent lines, and this, it may be noted, weakens the claims of *Peripatus* somewhat.

The arachnid question is, however, somewhat of a side issue, as the Crustacea alone are sufficient to show us that, if we admit *Peripatus*, we must break up the Arthropoda and depose the jointed leg from its present high place. It must be regarded as a product of convergent evolution or homoplasy. Another important point in this connection is noted by Mr. Hutton, namely, that the compound eyes of Insecta and Crustacea are in this case also a product of convergent evolution. The resemblance between them is very close, but I hope soon to be in a position to show that it is only superficial.

If, on the other hand, we take advantage of the weakening of the claims of *Peripatus*, due to the necessarily double, and therefore, possibly threefold origin of tracheæ, and cast it out from among the Arthropoda, I do not think that we gain much. By doing so we necessarily seriously weaken the absolute position of the jointed leg; for, though not elaborately articulated, the legs of *Peripatus* approach

sufficiently near the arthropod type to shew us the possibility of convergent evolution acting so as to produce jointed legs along different lines. The remaining classes of the Arthropoda are, further, by no means easily arranged. The Crustacea must be regarded as standing on their own base down to the lowest forms, and possibly, if the nauplius has any phylogenetic significance, down to much lower forms than exist at present. The whole series of Crustacea, however, give us no point from which the other Arthropoda can be derived. The characteristic specialization of the crustacean head resulting in two pairs of preoral appendages and three pairs of masticatory ones, would have to be undone to arrive at any of the other Arthropoda. It is of course possible here as elsewhere to imagine a hypothetical ancestral form with jointed legs from which the various classes can be derived, but is it necessary? Is the jointed leg a structure of such profound significance that we should let it trample on all other considerations? The gulf between a parapodium and such a leg is not so wide and deep as it appears at first sight, and must have been crossed more than once whichever position we assign to *Peripatus*. The change is one of obvious advantage in many ways, and given the formation of an exoskeleton, is almost inevitable. Further, the legs of the different classes have no points in common, except the one of being jointed. The legs of the Crustacea are traceable throughout to modifications of the one well-marked biramous type persisting through all kinds of functional changes, but no trace of this type, except a few extreme variations which might be used to prove anything, is seen in the other classes. Similarly, the legs of Arachnida and Insecta have very few points of resemblance. The number of joints, the relation of the anterior appendages to the mouth, the presence of antennæ (if these are to be considered as homologous with the other appendages) in the Insecta and Myriopoda, all afford marked points of difference.

One important point of morphological resemblance remains however, *i.e.*, the cœlom. The universal disappearance of this structure in Arthropoda wants explaining, and, if one is prepared to drop *Peripatus*, is an argument of no little weight for the phylogenetic unity of the group. If we retain *Peripatus*, its importance vanishes, since, as I have said, it is impossible to consider *Peripatus* as ancestral to the whole group, and it follows that the cœlom was suppressed independently in the different classes. It seems possible that this reduction of the cœlom may be directly associated with the acquisition of jointed legs, and of a body formed of a series of inflexible rings, the consequent enormous muscular development tending to encroach upon it. The excretory function usually associated with the cœlom being partly taken over by the formation of the cuticle, would permit of this encroachment on the cœlom, and would account for the reduced number of nephridia. The whole question of the cœlom in the Arthropoda is, however, wrapped in profound obscurity.

In conclusion, once one fairly faces the Arthropod question, the "Phylum" seems to disintegrate into a number of separate groups. Three assemblages seem quite natural, (1) the Crustacea, (2) the Arachnida, and (3) the Tracheata Antennata (Insecta, Myriopoda, and very probably *Peripatus*). The relations between these are yet to be determined, and the common ancestor is almost certainly to be sought for among animals which are not arthropod in structure.

MALCOLM LAURIE.

St. Mungo's College, Glasgow.

THE question raised by Professor Hutton seems to be twofold: (1) Are the Arthropoda mono- or poly-phyletic? (2) If polyphyletic, is it justifiable to speak of them comprehensively under one title? Touching the first question: it is generally admitted that the key to arthropod morphology is to be sought amongst the chætopod worms. It is also admitted, I take it, that among the arthropods those which stand nearest to the chætopods are the Prototracheata (*Peripatus*), on the one hand, and the Phyllopod Crustacea (*Apus*), on the other; also that *Apus* is the most primitive type of the great aquatic branchiate division, the Crustacea; and that *Peripatus*, if associated with any classes at all, must be looked upon as most nearly related to the ancestral type of the terrestrial tracheate division, comprising insects, centipedes, and millipedes.

This conclusion leads to the inquiry whether the *Peripatus*-like ancestor is derivable from the *Apus*-like ancestor or *vice versâ*; or whether the two are traceable to a common hypothetical form, possessing the characteristic arthropod features, and thus standing apart from the Chætopoda. The answer that is given to these questions will be the answer to Professor Hutton's query concerning the plurality of origin of the Arthropoda.

To my mind the answer must be in the negative in each case; whence it follows that we must go back as far as the Chætopoda to find a connecting link between the Tracheata and Branchiata. Moreover, it appears to me that the same result will be reached if we select any other crustacean instead of *Apus* as the primitive type, and if we discard *Peripatus* as the ancestor of the Tracheata, and choose some other form, real or imaginary, to take its place.

It should be borne in mind, however, that there is no connecting link either between *Peripatus* and the Chætopoda, or between *Apus* and the Chætopoda. The two forms seem, in fact, to have travelled from the chætopod starting-point along parallel roads; and being furnished with the same material in the way of parapodia, not to mention other organs, they have utilised them in the same way in response to similar needs, converting those in the neighbourhood of the head into feelers and jaws, and others farther back into more perfect organs for progression. It thus becomes possible, owing to the disappearance of the intermediate stages, to give a diagnosis of

the primitive Crustacean, and of the primitive Tracheate, which will serve to show how, in company with the more specialised forms usually associated with them, they may be distinguished from all known bristle-footed worms. This is one of the reasons why, in spite of my belief in the plural origin of the classes signified, the retention of the term "Arthropoda" commends itself to me as expedient. Another is, that, owing largely to our ignorance of that special branch of morphology which deals with the nerve-segments of the head (cerebral neuromeres) and the homology of the cephalic appendages, we are not yet in a position to decide as to the number of sections into which the old group should be severed.

Although the question of the classification of the Arthropoda is somewhat outside the limits of the present inquiry, I should like to be permitted to add that, in my opinion, the following main sections may be recognised:—I. PROTOTRACHEATA or MALACOPODA, for *Peripatus*, which occupies an isolated position, though most nearly related, in my opinion, to the Chilopoda of the section Opisthogoneata. II. TRACHEATA, with the subdivisions: A, PROGONEATA (Classes: Diplopoda, Pauropoda, Symphyla); B, OPISTHOGONEATA (Classes: Chilopoda, Hexapoda). III. BRANCHIATA or ACERATA, including: A, CRUSTACEA; B, GIGANTOSTRACA (Trilobita, Eurypterida, Xiphosura); C, ARACHNIDA; D, PANTOPODA (Pycnogonida).

The terms Branchiata and Acerata seem to me to be equally open to objection as applied to the third assemblage; but for the present they may pass. The Pantopoda are here included, because their affinities appear, on the whole, to be near the Arachnida. The latter are placed near the Gigantostraca, because, in my opinion, the close resemblance between the Eurypterida, not to mention *Limulus*, and the scorpions is inexplicable on the theory of convergence during descent under totally different conditions of life from forms so remote from both as the Chætopoda; though this is what my friend, Mr. Bernard, would have us believe. As for the Linguatulina and Tardigrada, the evidence that they belong to the Arthropoda seems to me far from conclusive.

R. I. POCOCK.

British Museum (Nat. Hist.).

IT may be convenient to mention that the first author to whom Professor Hutton refers is, not Philipp Franz von Siebold of Japanese celebrity, but Carl Theodor Ernst von Siebold, who established the Arthropoda in the Text-book of Comparative Anatomy, published by himself and H. Stannius 1845–1848. The second volume appeared in 1845, but the first, for which Von Siebold is responsible, not till 1848. It is this latter which is occupied by the invertebrate animals. Among them the Arthropoda are the fifth principal group, containing Crustacea, Arachnida, Insecta, the Myriopoda being included under the Crustacea. Fritz Müller's opinion that the

four classes, Crustacea, Insecta, Myriopoda and Arachnida, are all branches of a common stem, is given in the long and interesting foot-note which he contributed to Dallas's translation of his "Für Darwin" in 1869. If this opinion be correct, "it is evident," he says, "that the water-inhabiting and water-breathing Crustacea must be regarded as the original stem from which the other terrestrial classes, with their tracheal respiration, have branched off." The tracing of the pedigree must be sought elsewhere, for in the conclusion of "Facts for Darwin," after glancing at a suggested connection between the Rotatoria and Crustacea, Fritz Müller says, "But I can see nothing certain. Even towards the nearer provinces of the Myriopoda and Arachnida I can find no bridge. For the Insecta alone, the development of the Malacostraca may perhaps present a point of union."

It is obvious that if the primitive arthropod is to be regarded as anything like a crustacean nauplius, *Peripatus* cannot be a primitive arthropod. Still it might be an arthropod, without being a primitive one. Primitive and tracheate seem to be scarcely compatible terms. Among the Crustacea some of the terrestrial Isopoda have appendages furnished with tracheæ. It would be very difficult to believe that in the Isopoda such a character was primitive and not a late acquisition. Considering the remoteness of the geological time at which we find the classes of Arthropoda already distinct from one another, there are but dim hopes of our ever positively discovering the point at which their lines of ancestry meet. The "probable antiquity" which Mr. R. I. Pocock (*Journ. Linn. Soc. Zool.* vol. xxiv., p. 518, 1894) attributes to *Peripatus*, *Peripatoides*, and *Peripatopsis* may be indefinitely great, but hitherto it has told us nothing of the form from which they started on their peripatoidal career. We cannot tell how many stages have been made in advance, nor how many backward. Although this anomalous group has only been seriously studied for a short time; there are already three names for it at our disposal, Malacopoda, Onychophora, and Prototracheata, of which the last seems the least suitable because it implies a yet unproved hypothesis. But there is surely no urgent necessity to decide whether the group belongs to one or other of the primary divisions of the animal kingdom as hitherto accepted. Whether it be left standing on the outskirts of the Vermes or of the Arthropoda, in the modern activity of research there is always a chance that fresh light will be thrown on its affinities, and its position then assigned on reasonable grounds, and not by premature guessing.

T. R. R. Stebbing.

Tunbridge Wells.

Summary.

Though the opinions as to the constitution of the Arthropoda, advanced by the specialists who have kindly replied for us to Captain Hutton, are as diverse as possible upon many points of taxonomy,

there is tolerable unanimity touching the isolated position of *Peripatus*. Even on this subject, however, there are gradations of opinion, from that held by Mr. Stebbing, who would wait for further evidence before expelling the Malacopoda from the rest of the Arthropoda, to that of Dr. Hansen, who boldly maintains that *Peripatus* is nothing but a specialised chætopod. Mr. Carpenter and Professor Claus, on the other hand, regard it as a type that early diverged from a common arthropod stock, structurally in advance of the grade of organisation characteristic of the higher Annelida. These two authors therefore regard the Arthropoda as a homogeneous, monophyletic assemblage, while Messrs. Kingsley, Laurie, Pocock, and Bernard, who hold that there is no common ancestor for *Peripatus* and, say, the Crustacea nearer than the Chætopoda, are compelled to conclude that the "sub-kingdom" is polyphyletic. This also appears to be the view of Dr. Jaworowski, and also, as we have good reason to suppose, of Dr. D. Sharp. The balance of opinion seems therefore to be in favour of a plural origin for the Arthropoda.

Concerning the relationship of the various classes it is noteworthy that Messrs. Claus, Carpenter, Bernard, and Kingsley admit no affinity between *Peripatus* and the Tracheata Antennata, while Pocock and Laurie lean to the opinion that the two may be related, an opinion which was long ago tacitly expressed by Lankester when he established for them his group Ceratophora.

Again, Mr. Bernard seems to stand alone in his view as to the independent annelid origin of the Arachnida, Messrs. Claus, Kingsley, Pocock, Laurie, and Carpenter placing this class in close proximity to the Xiphosura, as was originally done by Lankester. But, while Pocock, Bernard, and Claus consider the Xiphosura to be the lineal descendants of the Trilobita, Professor Kingsley places the latter alongside the Crustacea and the Xiphosura at a distance under the Arachnida. This tangle of opinion is still further increased by the association of the Trilobita with the Crustacea by both Mr. Bernard and Mr. Pocock, and the separation of the two as of independent origin by Professor Claus. Mr. Laurie preserves a judicious, but provoking silence on the topic of the Trilobita.

Most of our contributors speak of the Myriopoda as a recognisable and natural assemblage; but while Messrs. Pocock and Kingsley repudiate the notion and classify the Chilopoda with the Hexap da, Kingsley does not admit the affinity that Pocock thinks traceable between the Diplopoda and the two classes now mentioned.

The opinions of Drs. Hansen and Jaworowski, though mentioned in connection with *Peripatus*, differ so entirely from those of the other contributors as to be of necessity treated apart. Hansen, who, perhaps wisely, has nothing to say concerning descent, regards the Arachnida and Pantopoda as isolated classes of the Arthropoda, and the Crustacea, Myriopoda, and Insecta as closely allied, this unique conclusion being based upon the structural resemblances in the

gnathite known as the mandible, which is assumed to be homologous in those classes. Lastly, Jaworowski declares it to be a self-evident fact that the Arthropoda are descended from worm-like ancestors possessing sac-like pouches for breathing air. The opinion of the majority, however, is decidedly in favour of their descent from aquatic non-tracheate organisms.

Captain Hutton deserves the thanks of zoologists for having elicited the opinions of so many well qualified to speak on this interesting question. But we should like to be present when he next lectures to his students on this subject, to hear how he has managed to reduce these varied views to dogmatic order.

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III.

Have Fish a Memory?

A CHAPTER IN THE EVOLUTION OF THE BRAIN.

FEW contrasts are more striking than that between the nebulous metaphysical psychology of former generations and the modern development of that science on physiological lines. It would now appear that comparative anatomy is likely to prove of inestimable value in its further advance; and for this, science is largely indebted to Professor Ludwig Edinger of Frankfurt, whose labours in this field, extending now over many years, have already gone far to establish a veritable science of comparative psychology. Professor Edinger asks us to publish an appeal for information on the subject of the existence of memory in fishes. The importance of this question, perhaps at first sight not very apparent, is clearly shown in the light of an address which he has published in the *Allgemeine Medicinische Central Zeitung* for 1896 (Nos. 79 and 80) on the Development of the Brain-tracts in the Animal Series, which lies before us, and a short account of which will not only prove of interest to our readers, but may help to stimulate a response to the question propounded by its author.

His contention is briefly this. No hard and fast line exists between the mental capacities of the lowest and highest vertebrates. We ought to be able to trace the various transitional stages, and by the light of comparative anatomy and histology to assign their true psychological value to the different nervous structures which make their appearance in ascending the animal scale. But to do this we require much new and detailed information as to the mental capacities of various groups of animals; and in collecting this it must not be taken for granted that apparently deliberate and purposeful actions are in reality due to conscious volition as we understand it in the higher animals. Professor Edinger denies that in lower organisms the lower nerve-centres, which alone are present, can fulfil the same functions as the higher centres, which are first to be found higher up in the animal scale. He regards the appearances of conscious volition shown by such invertebrates as a sea-anemone or an earthworm as purely reflex: it is the observer who is apt to employ too "anthropo-psychic" a standard in their interpretation. It has been demonstrated by Retzius that the ventral nerve-cord of an earthworm contains a complete and elaborate reflex mechanism:—afferent sensory fibres which, after sub-division, are connected with the dendritic processes of

the great ganglion-cells, whence arise the motor nerves, and also nerve-cells which seem from the distribution of their processes fitted to fulfil an associative function between the neuro-muscular apparatus of adjacent segments or metameres. A precisely similar fundamental mechanism is present in the spinal cord of all vertebrates from the lowest to the highest, and this mechanism must alone suffice for all the various and often purposeful movements which a decapitated frog, duck, or rabbit can perform, and indeed for the whole range of capacities possessed by *Amphioxus*. A direct influence of the brain upon the affairs of the cord does not exist in the lowest vertebrates, though already in sharks association-tracts connect the cerebellum and mid-brain with the cord. In passing upwards, such association-tracts develop more and more, till in mammals we find the pyramidal tracts connecting the cord with the cerebral cortex itself. At the upper end of the cord, where the cranial nerves arise, the association-tracts are immensely developed, and this, with the size and importance of the nerve-nuclei there situated, gives rise to the medulla oblongata.

The cerebellum first appears in cyclostomes (the lamprey and hag-fish), but its development depends chiefly on physiological causes: great swimmers have great cerebella, *e.g.*, sharks; and swimming reptiles have one twice as big as have reptiles which live on land. The lateral lobes are comparatively late acquisitions.

Next to the spinal cord, the mid-brain is the most constant in the whole animal series: in all embryos, and permanently in lower vertebrates it is a large and conspicuous organ, and it receives the main part of the terminations of the optic nerve, as well as the terminations of most of the sensory association-tracts from the cord. In animals with large eyes, *e.g.*, bony fishes and birds, it has an unusual development.

The fore-brain consists essentially of an olfactory apparatus, a basal ganglion, and a pallium. The olfactory apparatus is always basal, and has already reached a high grade of development in fishes, though in different classes it varies with the mode of life, being almost absent in birds. The function of the corpus striatum, though as yet unknown, is manifestly important. It is the pallium which is the important region from the point of view of psychical development: the evolution of other parts of the brain is insignificant in comparison with that here to be seen, ending in the colossal cerebral hemispheres of man. For simple movements, for the primary appreciation of sensory impressions, a simple apparatus serves everywhere alike, but higher associative mental activity first becomes possible with the development of this part of the fore-brain. In earliest rudiment, it exists as a thin plate of epithelium in selachians, ganoids and teleosteans. In amphibians it is definitely of nervous texture, and in reptiles it first takes on the form of a distinct cerebral cortex. The primitive cortex seems to have demonstrable relations with the

olfactory apparatus only: it is merely a centre for olfactory associations. The differences between a fish and a reptile must be that the latter is able to store, associate and compare its olfactory impressions—the former, not. In passing up the animal scale new centres are added, one at a time, and so piece by piece the whole cerebral cortex is built up. It is for the future to trace the details of the process, but already we can follow the "installation," as it were, of the cortical visual field. In fishes, in young animals, in a new-born child, the retinal fibres end in the roof of the mid-brain. In the second month, as the child begins to acquire visual experience, fibres develop from the mid-brain to the occipital region of the cerebrum, and other anatomical connections quickly arise between this cortical visual field and other regions of the brain. The child now begins not merely to see, but to perceive and correlate visual impressions. This visual tract from the mid-brain to the cortex is wholly absent in lower vertebrates. Fishes are readily deceived by the angler; even a hungry frog or snake may fail to see its prey unless the latter moves, or is detected by smell. But birds have this tract well developed, and they clearly perceive and store impressions. It takes a good scarecrow to deceive a sparrow. Just as the lower vertebrate depends on smell for the supply of its requirements, so the bird relies on vision. It is of course amongst mammals that the cerebral cortex acquires its chief complexity, and that the manifold development of association tracts gives rise to the white substance of the hemispheres.

The bearings of Professor Edinger's question will now be more clearly grasped. The animal kingdom got on for a long while without a cerebral cortex: What capacities are possible to the lower primary centres unaided? What, in the domain of a smell, can a reptile accomplish more than a fish? What, in the domain of vision, can a bird do which a reptile cannot? What new faculties has a mammal associated with the new cortical areas which it possesses? Even in man we require accurate topographical studies of individual brains in relation to individual gifts and faculties. It is evident that in the answers to such questions as these, psychology may attain a far more secure basis than any which has yet been provided for it.

The particular case put by Dr. Edinger is this: There is a general opinion, that fish have some sort of memory, that they can recognize people, know how to find or to avoid places, where they have formerly had some experience; that fish, which have once escaped the rod, know the bait, etc. It is highly desirable that all observations of this kind should be collected in the interest of comparative psychology. The reason is that till now we believed the function of memory to depend on the action of the brain-cortex. All experience in man and in the higher animals has led to that conclusion. But it has in recent years been proved that fish have no brain-cortex at all; they are the only existing vertebrates without one. Now, if we could prove beyond the possibility of doubt, that

fish really have a memory, that they gain experience and can make use of it, then we should have to give up the usually accepted opinion that memory has its seat in the brain-cortex. Therefore, it is extremely important to have an entirely new collection of observations. Dr. Edinger begs all, who are in any way interested in fish, and especially anglers and naturalists, kindly to send an account of any observations they may have made to him at 20 Gaertnerweg, Frankfurt a. M. He requests them particularly to take nothing as a well known fact. The smallest observation may prove of the greatest importance.

IV.

Additions to our List of Palæontological Specialists.

IN our December number (vol. ix., p. 361) we published a list of specialists willing, under the conditions there stated, to determine fossils for stratigraphers. Our first duty now is to thank our contemporaries for the favourable terms in which they have spoken of our action, and various correspondents whose commendations we have unfortunately no space to publish. Better, however, than these kind words, is the fact that a slight prejudice which a few had at first against the scheme seems to be disappearing, so that we can now request our readers to add the following names to the former list.

ARTHROPODS—		
Ostracods .. Phyllopods ..	From any horizon	T. Rupert Jones.
Eurypterids .. Limuloids .. Scorpions or other Arachnids.	" "	Malcolm Laurie.
Trilobites	Silurian	Philip Lake.
COELENTERATES—		
Graptolites	From any horizon ..	Gertrude L. Elles. Ethel M. R. Wood.
FORAMINIFERA	" "	T. Rupert Jones.
PLANTS	Newer Tertiary and Quaternary	Clement Reid.
"	Palæozoic (if internal structure is preserved) ..	D. H. Scott.

Our readers are also asked to correct the name of one of the genera in which Miss Donald is specially interested, from *Aelisina* to *Aclisina*.

Mr. E. J. Garwood, Secretary to the British Association Committee appointed for the study of life-zones in the British Carboniferous rocks, asks us to call attention to the Report of that committee. This we have already done more than once. We may, however, add that the report contains a list of eleven palæontologists asked to co-operate with the committee "to identify such fossils as may be submitted to them." We did not allude to this list for the following reasons: first, we received authoritative information that some of the naturalists mentioned had declined the proposals of the committee in whole or part; secondly, four of their number definitely

refused to allow their names to appear in our list, fearing to be overwhelmed with frivolous applications, a danger we have done our utmost to guard against; thirdly, the names of four of these gentlemen are in our list; lastly, Mr. Garwood's reply only reached us on December 13th. We are still unable, for reasons indicated, to quote this list of Mr. Garwood, but we have pleasure in referring our readers to it, either in the *Geological Magazine*, November, 1896, p. 520, or in the forthcoming *Report* of the British Association.

We should also recommend geologists who do not find their wants provided for in our list, to refer to the annual volume of the Palæontographical Society, where they will find a goodly list of monographs in course of preparation or publication, the authors of which *ought* to be glad of material.

The *Naturalist* does us the honour of making the following criticism:—"We may point out serious omissions in these conditions, and we think it ought to be made clear that in return for the full acknowledgment to be given of the specialists' assistance, the specialist himself should undertake to give a correspondingly full acknowledgment to the sender of the material on every occasion of publishing notes on the specimens, and also that the right of priority in publication is also to be reserved to the sender." With the former condition we fully agree, but as regards the latter we must point out that it would lead to a danger we are doing our best to fight against, namely, the preliminary publication of imperfect descriptions, or even of hosts of *nomina nuda*. Of course we take it for granted that our specialist friends have still some sense of honour left, and will not rob the stratigrapher of all his new facts and conclusions.

SOME NEW BOOKS.

The British Museum Catalogue of Madrepores.

Catalogue of the Madreporarian Corals in the British Museum (Natural History). Vol. ii. The genus Turbinaria. The genus Astræopora. By Henry M. Bernard. Pp. iv., 106, plates xxxiii. Published by order of the Trustees. London, 1896. Price 18s.

Following upon the late Mr. George Brook's monographic catalogue of the genus *Madrepora*, comes the present volume, dealing with two genera of perforate corals, together including a far smaller number of species than the single genus *Madrepora*. Mr. Bernard has proved a worthy successor to Mr. Brook, and in compiling this catalogue has followed with tolerable closeness the lines laid down by his predecessor. The diagnoses of both genera and species have been framed with evident care, while the illustrations are numerous and very good of their kind. The collotype reproductions of photographs, introduced with such success by Mr. Brook, have been used in illustration of the entire specimens and show the habit of the colonies very well. Mr. Bernard, however, has improved upon the method of his predecessor, by introducing a number of lithographs showing details of corallites: these drawings, which illustrate the characters used to diagnose the different species, are of great value, as may well be understood by anybody who has attempted to determine the species of a coral from verbal diagnoses alone.

Every student of corals knows that they are protean in their variety, and that there may often be found, on one and the same colony, calicles of diverse and perplexing forms. This difficulty is emphasized by Mr. Bernard, who gives an account of the variations of each component of a calicle, and shows that there is no single character to be relied upon as a sure mark of specific distinctness. Finally, he complains of the restraint of the Linnean system, and prophesies a time when we shall have recourse to some other conception than that of a species. Such a complaint is scarcely in keeping with the progress of our ideas, for there are few nowadays who regard a species as necessarily fixed and invariable, or as varying only within very narrow limits. All Anthozoa present the most perplexing gradations of character, and the Zoantharia do not afford the same easy means of identification as do the Alcyonaria, for there are no spicules of characteristic and constant form. In point of fact, Mr. Bernard has in his arrangement expressed very well the modern conception of a species. He has selected certain leading features (in this case the habit of growth is most relied upon), and he has arranged his collection in groups about selected individual specimens chosen as types. Each group constitutes a species. The species, as thus arranged, will have a greater or less value according to the skill with which the author chooses his type-specimens, and his instinctive aptitude for associating with them those forms which most truly resemble them. By "most truly" we mean in the greatest number of particulars.

In selecting the habit of growth, that is, the mode of budding, as the basis of his classification, Mr. Bernard has done the best that a man can do who relies on external characters; still, he seems but little satisfied with his choice, and has a decided hankering after the calicular diagnoses of Milne-Edwards and Haime. His method, indeed, leaves something to be desired, as will be shown further on, but it would not be improved by reversion to a method which extended trial has shown to be unsatisfactory. It strikes one that he is not consistent when he says (p. 20) "that the form of the corallum is not altogether independent of the form of the individual calicle, the corallum itself being but an aggregate of individual calicles." He has shown just before that the characters of calicles may vary according to their positions on the corallum, so that the corallum would appear to determine the calicle rather than the calicle the corallum. The growth of the corallum, again, is modified by local conditions, and the growth of the corallum modifies the calicles. In short, corals are extremely sensitive to the influence of environment, and are highly plastic. Hence the difficulties of classification.

In order to account for certain structural features, Mr. Bernard has called into being a new influence, that of the gravitation of the nutrient fluid in the cœnenchyme. He would have it that the nutrient fluid runs downwards, is most abundantly supplied to the lowest portions of the colony, and that excessive growth takes place there, leading to immersion of the calicles. This is not quite a happy conception: the region of most active growth is in the *uppermost* portions of a normal crateriform colony, and where growth is greatest the supply of nutriment must also be greatest. The gravitation theory is quite inapplicable to numerous acrogenous forms of perforate corals, *e.g.*, *Madrepora*, and the phenomena which the theory is invented to explain are better accounted for on other grounds, viz., that in the older parts of the colony such growth as takes place is limited, both in amount and direction; it can only result in the filling up of the cavities or in a thickening of the mass; growth in the latter direction leads to immersion of the calicles.

It may be doubted, too, whether Mr. Bernard has formed sufficiently accurate ideas as to the morphological value of the parts of the corallum. Turbinarians, according to him, are purely cœnenchymatous corals, and he speaks of the skeleton of the polyp as being built up entirely of the septa and their *synapticular* connections. Surely the use of the term "synapticular" is unwarrantable; and the references to cœnenchyme and theca, or the absence of theca, betray a want of precise conceptions respecting these structures. Those who are acquainted with synapticulæ as they occur in the Fungidæ or in *Stephanophyllia formosissima* will not hesitate to deny their homology with the trabecular pieces which unite the septa in a perforate coral such as *Turbinaria*. Compare again Von Koch's account of the development of the skeleton of *Astroides calycularis*, Dr. Fowler's description of the anatomy of *Rhodopsammia parallela*, his description of *Turbinaria mesenterina*, and a section made through any species of *Madrepora*. In *Astroides* and *Rhodopsammia* one speaks of a theca formed by union at many points of the peripheral ends of the septa; the theca is porous, permeated by canals, which communicate with the cœlentera of the polyps. In *Madrepora* and *Turbinaria* the exsert portions of the calicles have a structure to all intents and purposes identical with that of *Rhodopsammia* and *Astroides*. The cœnenchyme is but an extension of the perforated theca; it is permeated in the same manner by a canal system; and the characters of the corallum,

as seen in section, are indistinguishable from those of *Rhodopsammia*. In fact, the porose theca has become much extended; buds have been formed from its canals; and these buds have become new polyps, each with its own theca, which has extended in a like manner and given rise to new buds. Cœnenchyme is but a name for the spongy tissue intervening between the *cavities* of the polyps. If each cavity has its own spongy wall, this wall is called a theca, if the spongy walls are fused to form a mass, the mass is called cœnenchyme. The names are hallowed by use; yet they do but signify two phases of one and the same structure. To say, as Mr. Bernard does, that the Turbinarians are *athecalia*, is to reject all those concepts of the nature and development of a "theca" that have been formed as the result of the labours of Von Koch, Fowler, and others.

Such loose conceptions as those here criticised result from too exclusive attention to external features, to the neglect of the study of the evidence afforded by sections. For this Mr. Bernard can hardly be blamed. The system of the British Museum demands diagnoses founded on external characters: considerations of internal structure find no place in its catalogues. But the external characters are the expression of an internal structure; and if the latter is not thoroughly studied and mastered, the external features are apt to be wrongly interpreted. Forms like corals, whose superficial features vary, as has been shown, with every change of circumstance, cannot be truly classified except as the result of a close study of the intimate structure which remains fairly uniform through all those changes impressed upon the colony by the action of external influences.

G. C. BOURNE.

THE CRAFT OF THE TAXIDERMIST.

ARTISTIC AND SCIENTIFIC TAXIDERMY AND MODELLING. By Montagu Browne, 22 plates and 11 text figures, 8vo. Pp. xii., 463. London: Adam and Charles Black, 1896. Price £1 1s.

IT would seem that brighter days are in store for the long-neglected art of taxidermy. To the present time its existence has depended largely upon the whims and caprices of a small minority of the general public, whose influence on the whole has perhaps been the reverse of beneficial. That the case should have been otherwise was hardly to be expected, so long as the end and aim was no more ambitious than the preservation of trophies of the chase or defunct pets, which were entrusted to men who were as devoid of all artistic sense of form and colour, as they were ignorant of the merest rudiments of anatomy. The last few years, however, have witnessed a decided change for the better. This may be traced to the zeal of private collectors like the late Mr. Booth, of Brighton, and to the emancipation of museums from the fetters of tradition, and their re-organisation on a scientific basis. So long as the museum remained a species of "Old Curiosity Shop," so long was taxidermy doomed to "live desiring without hope."

Mr. Montagu Browne has ever been a champion of the new *régime*, and has given a proof of his earnestness by his contributions to practical taxidermy. His last volume now before us is decidedly his best, aud represents matured thought and experience. As curator of the Leicester Museum, he has ever contended that taxidermy should receive official recognition, and that the museum should be its official home. Who will say him Nay?

In the space of some 400 pages Mr. Browne conducts us through the whole domain of taxidermy and modelling. The history of the origin and progress of taxidermy is compressed into some 18 pages.

Then follow chapters on methods of preservation and modelling compositions, collecting Vertebrata and Invertebrata, the skinning and setting up of mammals, birds, and fishes, etc., by various methods, the modelling of flowers, foliage, etc., and the arrangement of animals in groups in natural surroundings.

These chapters are, on the whole, all that could be desired; here and there, however, we would venture to differ from the author. For instance, we are far from sharing his enthusiasm for bichromate of potash as a preservative fluid. Throughout the book methylated spirit is described as methyl-alcohol, which is, of course, not correct. The formulæ for modelling waxes will be found very useful, but No. 63 might be still further improved by doubling the amount of plaster; the ochre may be omitted at discretion.

The directions for "setting up" mammals by means of wooden models or mannikins will be particularly welcome. It is, however, a matter for real regret that these are not supplemented by a few illustrations from the author's previous works, as well as by one or two after Hornaday. This method is overshadowed in the present volume by a system of "casting from the actual subjects and their reproduction in paper." In the hands of a master of the craft this process may be made to work successfully, but he must be a master, or a very wooden effect is certain to result. Once the model is made, alteration is almost out of the question. We think this latter method should not be attempted, if at all, until after the 'mannikin' has been thoroughly mastered.

No one who has ever tried the system of skinning birds under the wing will ever again revert to the ancient method of removing the body through the breast. The latter system is dismissed in the present volume as utterly bad, whereas in the author's former books, this method received the chief share of attention. Perhaps the tyro would have been better advised if he had been bidden to choose such a bird as a starling or gull, instead of a pigeon, for his first lesson, since from the reviewer's experience, the pigeon is one of the most difficult birds to undertake. It is hard to skin, and hard to mount.

On p. 194 Mr. Browne treats of the method of setting up birds on "scientific principles." The method in question is the same as that adopted for large mammals, namely, the employment of a paper model made from a cast of the body after removal from the skin, which is then to be stretched over it. Experience may gain it a place in our affections, but certainly we are not at all disposed in favour of it. Later, a process is explained by means of which the whole skeleton is retained and made to form a framework for the support of the skin. This, as an object lesson for demonstrating the relation of the bones, and the way in which the skeleton determines the outer form, is admirable, and nothing could be more instructive or helpful to a beginner or one ignorant of anatomy. We commend it to the notice of the professional "bird-stuffer," who, as a rule, is about as ignorant of these matters as can well be. "Many specimens have been set up by this method, which is so easy of accomplishment, and gives such fine results, that in the Leicester Museum, in future, all mammals and birds, when not modelled, will be executed by this process." As Mr. Browne points out, there is nothing new in this; it has been tried and discarded, and as a matter of every-day practice will be rejected now, and it is best so. This method, as well as that of paper models, is a dangerous one, and inimical to the progress of taxidermy. It would scarcely be possible to invent a system which demanded less of the operator. There should be no necessity for such shifts and

props if the operator is, as Mr. Browne would have him, an artist, and familiar at least with surface anatomy. What sculptor would think of modelling clay upon a skeleton, when making the preliminary model for some statue?

All will agree with the author in his contention that no "taxidermist ever has mounted, or ever will mount, a fish, amphibian, reptile, or cetacean even decently, or with more than a remote approximation to nature by any of the resources of their art known to them." Consequently, we must resort to plaster-of-Paris models, full details concerning the making of which will be found in this book. We are also offered a choice between reproduction by means of layers of paper, or paper-pulp worked into the plaster mould and lifted out when dry. With both these methods Mr. Browne claims to have achieved great success. Yet a third method—that of models in modelling glue. All these are new, and we await the test of time with some curiosity. Certainly a model in glue of a grass snake which is now exhibited in the Leicester Museum leaves nothing to be desired. Many of the invertebrates, *e.g.*, cephalopods, have been subjected to the glue model test.

The chapter devoted to casting and modelling from natural foliage is very fully and lucidly handled, and will prove invaluable to many.

The concluding pages of the book deal with the formation of "Pictorial groups of Mammals, Birds, etc." Here we are treated to a charming word-picture of a Lincolnshire marsh in midsummer, which, however, jars sadly with the accompanying cold-blooded account of the slaughter of the poor unsuspecting birds who had made this quiet and beautiful spot a temporary home.

In conclusion we may add that the illustrations are for the most part extremely good and to the point. In spite of the one or two drawbacks to which we have drawn attention, the work is, and indeed will long remain, the very best and most authoritative treatise we possess on practical taxidermy.

W.P.P.

Reproduction in Algæ and Fungi.

DIE BEDINGUNGEN DER FORTPFLANZUNG BEI EINIGEN ALGEN UND PILZEN. Von Dr. Georg Klebs. 8vo. Pp. xviii., 543, with 3 plates and 15 figs. in text. Jena: G. Fischer, 1896. Price 18 marks.

WE have before us the results of very numerous observations and experiments forming the "special portion" of a general work on the physiology of reproduction in lower organisms. The second part, which will appear later, will be based on the results detailed in the first, as well as on facts already known. We heartily commend this plan of work. When a man has filled five hundred pages of text with an account of his own observations, his general conclusions will demand a certain respect and consideration. Whether they ultimately prove to be right or wrong is another question. So far, the author has put before us in a plain straightforward way the conditions which accompany or seem to govern reproduction, both sexual and asexual, in some common species of fresh-water Algæ and Fungi. It is open to anyone who is interested in this most interesting of subjects to repeat and verify or disprove his observations. *Vaucheria*, *Hydrodictyon*, *Protosiphon*, *Botrydium*, *Spirogyra*, *Œdogonium*, *Ulothrix*, *Conferva*, *Chlamydomonas*, and a few others represent the Algæ, *Eurotium repens* and *Mucor racemosus* are the objects of observation among Fungi.

A *resumé* of some of the work on *Vaucheria* will give an idea of

the whole. Let us take the asexual reproduction by means of zoospores in *V. repens* and *V. clavata*. The relations to this process of the following factors are considered in order, namely, (1) nutrition, (2) moisture, (3) light, (4) temperature, (5) chemical nature of the environment, (6) oxygen, and (7) currents of water. In the case of nutrition it was found that starvation, by growing in the dark, thereby stopping formation of carbohydrate, after a time prevented zoospore-formation, though growth still continued. Similarly, plants cultivated for a month in distilled water, though exposed to the light, grew slowly, but could not be induced to form zoospores. In the second case the alga was grown for some days in damp air. Under such conditions there arise from the threads which lie on the substratum, numerous branches, which, when surrounded by water, produce zoosporangia at their ends. In damp air the sporangia were never formed. The addition of water acts as a stimulus, and the transition must accordingly be sudden; if gradual there is no result. By exclusion of light, formation of zoospores was induced often after twenty-four hours only. Care must of course be taken that the exclusion of light is not of long enough duration to cause starvation. Experiments with different coloured solutions indicate that shutting off the yellow-red rays is especially favorable to zoospore-formation. Klebs has previously shown that the process in question ceases below 3° C. and above 26° C. He also finds that temperatures between 10° C. and 20° C. are equally favourable to zoospore-formation, so that within these limits temperature ceases to be a factor in any given experiment. Increase of the amount of oxygen in the water in which the plant is growing does not lead to production of zoospores. By transposing individuals from streaming into stagnant water, rapid formation of zoospores was induced; probably as a result of a diminution of the amount of dissolved oxygen. These few remarks give but little idea of the numerous observations occupying 80 pages of the text. For details, and for an account of the remainder, we refer our readers to the book itself, to which physiologists in general, and serious workers at Algæ and Fungi in particular, will, we feel assured, give a hearty welcome.

Names at the Zoo.

List of the Vertebrated Animals now or lately Living in the Gardens of the Zoological Society of London. Ninth edition (by P. L. Sclater). 8vo. Pp. xvi., 724, with 70 woodcuts. London: Printed for the Society, 1896. Price 4s. 6d.

The last edition of this valuable list appeared no less than thirteen years ago, so that the handsome new volume contains the records of twelve years (1883–95), arranged, as usual, in systematic order. The names of species recorded in the previous lists are also added, with references, so that the present work is practically an index to all the species of vertebrated animals received alive by the Zoological Society of London during the past thirty-four years. As regards the names used, Dr. Sclater has endeavoured to make as few changes as possible, "not being one of those advocates of priority who seek to carry out that useful principle without reference to grammar or common sense." Nevertheless, so far as the new series of British Museum Catalogues has advanced, he has adopted the various "revisions" of nomenclature without any question. This does not seem to us quite consistent—merely to use "revised terms" where they are ready to hand and need no seeking—but it at any rate displays great good sense to follow implicitly some standard work of

reference, when such exists and is universally accessible. It is, perhaps, too much to expect that the publications of our National Museum shall set a universal and permanent fashion in zoological nomenclature, but some consummation of this kind is devoutly to be wished.

NEW SERIALS.

THE *Monatschrift für Psychiatrie und Neurologie*, edited by Professor Wernicke, of Breslau, and Professor Ziehen, of Jena, and published at Berlin, began with the present year.

We learn from the *Daily News* that the Imperial Natural History Society of St. Petersburg intends to publish a flora, first of European Russia, and afterwards of Russia in Asia and the Caucasus.

Nature announces, though without stating the price or place of publication, the appearance of a new German weekly journal, devoted to pure and applied science, to literature and to art, *Die Umschau*, edited by Dr. J. H. Bechhold. The first number contains, among other articles, one on ethnology by Dr. Max Buchner.

Messrs. Schleicher Frères, of Paris, announce "L'Année Biologique: Comptes rendus annuels des travaux de Biologie Générale," published under the direction of Professor Yves Delage, with the assistance of a committee, to whom Dr. G. Poirault is secretary. The first volume deals with the literature of 1895, and is in large 8vo. We have not yet seen a copy.

Middlesex Hospital is starting a journal of its own.

SCRAPS FROM SERIALS.

THE *American Journal of Science* for January prints a letter by Dr. Webb, of St. Augustine, Florida, stating that the body of an immense octopus, 18 feet in length and 10 feet in breadth, has been washed ashore near that city. The arms no longer remain. Mr. A. E. Verrill, commenting on this, suggests a weight of four to five tons for the body and head, and states that the dimensions are larger than those of any of the well-authenticated Newfoundland specimens. He thinks that it may be a species of *Architeuthis*.

Mr. J. Collinson, of Wolsingham, Co. Durham, a well-known crusader against cruelty to animals, writes to the *Farmer's Gazette* for January 2, praising the refusal of the British Produce Supply Association to trade in larks, which are valuable friends to the gardener and farmer. The lapwing, too, as he quotes from the *Gardening World*, although of priceless value to agriculturalists on account of the vermin it destroys, is subject to ruthless persecution in spring by those who want its eggs for varnish or for food, in autumn, "by people who foolishly imagine they are eating something of a dignified and gamey nature."

The *Zoologist* for January is no longer edited by Mr. J. E. Harting, its editor of the last twenty years, who has been forced to retire in consequence of the pressure of official duties and other engagements. A worthy successor has been found for him in Mr. W. L. Distant.

The *Westminster Review* for January contains a curious article by Dr. Olindo Malagodi on the Psychology of Anarchist Conspiracies, which is of a reassuring nature, inasmuch as he contends that Anarchists are dreamers rather than fanatics. "It is the hyperæsthesia of their personality which forces them into their conflict with society. . . . Anarchists will continue to live in a small way on small deeds." We do not quite know why this *Review* criticises the Smithsonian Institution

for its manner of publication. It is not correct to say that "in order to obtain its Reports application must be made to members of Congress." No *bonâ fide* worker who applies direct to the secretary is likely to meet with refusal.

With its January number the *Photogram* enlarges its monthly issue to 32 pp., and introduces improvements in paper and printing. The present number contains an article by John Mills on spectrum photography for beginners; but even when the *Photogram* contains nothing that comes within the scope of NATURAL SCIENCE, it remains of interest and value.

The *Engineering News* for November 10, contains an account by John S. Denis of the photographic methods used in the Canadian Topographic Survey. This method is especially valuable in broken and difficult country.

Some results of the expedition organised in 1891 by Professor F. W. Putnam to the ancient city of Copan in northern Honduras, are given by C. C. Willoughby, of the Peabody Museum at Harvard, in the *Scientific American* for December 26, 1896. The article is illustrated with an admirable photograph of the amphitheatre with the temples in the background. The age of these massive and magnificent ruins cannot be certainly decided; they are, says Mr. Willoughby, unquestionably prehistoric, and the builders of this city belonged to the same civilisation as the constructors of the temples and pyramids of Yucatan (see NATURAL SCIENCE, vol. viii., p. 159, March, 1896).

We have received the Final Report of the British Association Committee on the Marine Zoology, Botany, and Geology of the Irish Sea. It consists for the most part of a list of the species recorded from that area. The Committee do not regard their work as finished, but feel that it will in future be carried on at Port Erin by the Liverpool Marine Biology Committee. We do not know whether this report is really published or not. It bears no imprint or date other than "Section D.—Liverpool, 1896," and no price or publisher's name is marked on it. Neither does it contain any reference to the Report of the British Association, of which the volume for 1896 is not yet issued. We should like to ask Professor Herdman, who is responsible for this Report, whether he considers such conduct to be consistent with the resolutions unanimously passed by the British Association Committee on Zoological Bibliography and Publication, of which we see that he is a member.

We have received the *Journal* of the Institute of Jamaica for July 1896, which was issued on November 10th. It contains the following contributions to the fauna and flora of Jamaica:—Dragon-flies, by G. H. Carpenter; Lizards, Scorpions and Myriapods, by J. E. Duerden, and the Melastomaceæ by W. Fawcett. We also find notes by H. L. Clark on the life-history of *Synapta vivipara*, a holothurian concerning which very little has hitherto been known; on the marine zoology of Kingston Harbour by J. E. Duerden, who also contributes an interesting note on phases in Jamaican natural history, dealing with the remarkable change in the fauna that took place after the introduction to the island of the mongoose in 1872. Originally brought to destroy the rats, themselves introduced by man, the mongoose also destroyed the majority of the snakes, lizards, turtles, and birds of the island, thus leading to an excess of many injurious insects, so that in 1882 a very bad state of affairs was chronicled. Now, however, it appears that in all parts of the island the mongoose is not so plentiful as formerly, and is beginning to be

attacked by those very ticks which it has permitted to multiply. The balance of nature is therefore being restored. The lizards, snakes, and birds are reappearing, and "the maximum influence both for good and for evil, of *Herpestes mungo*, is passing away in Jamaica."

Boletim do Museu Paraense contains in the 4th part of the first volume, a striking plate of the water-plant *Mourera fluviatilis*, one of the Podostemaceæ, illustrating a contribution to the botanical geography of Guiana, by Dr. Jacques Huber. Herein also F. Katzer announces the discovery by Dr. João Coelho of Silurian rocks, largely composed of sponge-spicules and containing graptolites, in the Maecuru valley. Such fossils had not previously been found in Brazil.

The *Cheltenham Examiner* has lately been publishing a series of articles on Cheltenham as a health resort, in which we are as much surprised as pleased to find one entitled "The Great Divide," a term which the writer here applies to the water-shed between the Severn and the Thames. He proceeds in thoroughly scientific, no less than popular fashion, to show how this has gradually moved backwards from the west, so that the Cheltenham area, instead of draining into the Severn, formerly supplied the Thames. The Chelt now flows westward, but there is evidence of a former river flowing along the same valley, but in an easterly direction, draining the westerly extension of the Cotteswolds, since worn away.

CEDRIC CHIVERS' "New Book List," the first volume of which has just closed, is a useful idea well carried out. But it will not be of much help to scientific men till someone of more technical knowledge is placed on its staff. A zoologist, looking at the subject index of no. 12, would infer that no zoological works had been published during December last; and yet the list itself contains Newton's "Dictionary of Birds, Part IV.," Cunningham's "Natural History of the marketable marine Fishes of the British Isles," Grant's "Handbook to the Game Birds, Vol. II.," "The Cambridge Natural History, Vol. II.," and five or six others that ought to have been referred to under zoology. So also Bailey's "Survival of the Unlike" is not alluded to under botany.

OTHER LITERATURE RECEIVED.

Fabrica Oris Dipterorum, H. J. Hansen: Copenhagen, Reitzel. Die Leitfossilien, E. Koken: Leipzig, Tauchnitz. Bibliography of more important Contributions to American Economic Entomology, pt. v., S. Henshaw: U. S. Dept. Agric. Traité de Zoologie Concrète, Y. Delage and E. Hérouard, vol. i. La Cellule et les Protozoaires: Paris, Schleicher. An Account of the Crustacea of Norway, vol. ii,, G. O. Sars: Bergen Mus. Catalogue of African Plants collected by Dr. F. Welwitsch in 1853–61, pt. i, W. P. Hiern: Brit. Mus. (Nat. Hist.) The Survival of the Unlike, L. H. Bailey: New York, Macmillan. A Dictionary of Birds, pt. iv., A. Newton: Black.

Undescribed Shrew of Genus *Sorex*, C. F. Batchelder; Rev. Squirrels of N. America; Skunks of Genus *Mephitis* of E.N. America; New Mammals from Indian Territory and Missouri, O. Bangs; *Romerolagus nelsoni*, n.g. and sp. of Rabbit, C. H. Merriam; *Proc. Biol. Soc. Washington*. Entwickelung d. sogen. Lungen bei den Arachniden, A. Jaworowski: *Zeitschr. f. wiss. Zoologie*. Entwickelung des Spinnapparates bei Trochosa &c., A. Jaworowski: *Zeitschr. f. Naturwiss.* Instruction in Natural History in the Jardin des Plantes, Paris, C. Earle: *Science*. On the Bio-Geography of Mexico. &c., T. Townsend: *Trans. Texas Ac. Sci.* N. American Oligochæta, F. Smith: *Bull. Illinois State Lab. Nat. Hist.* Human Relics in Drift of Ohio, E. W. Claypole: *Amer. Geol.* On the Teeth of Mazodus; On the Structure of some Palæozoic Spines from Ohio, E. W. Claypole: *Proc. Amer. Micr. Soc.* Insects affecting the Cotton Plant, L. O. Howard: *Bull. U.S. Dept. Agric.*

Nature, December 17, 24, 31, January 7, 14. Nature Notes, Jan. Science Gossip, Dec., Jan. Naturalist, December, January. Westminster Review, January. Victorian Nat., Nov. Knowledge, January, Photogram, January. Irish Naturalist, January. Scott. Geogr. Magazine, January. Revue Scientifique, December 12, 19, 26, January 2, 9, 16. Feuille des jeunes Naturalistes, January. L'Anthropologie, November–December. Naturen, January. Naturæ Novitates, Nov. (22), Dec. (24). Literary Digest, December 12, 19, 26, January 2, 9. American Journ. Science, January. Science, December 11, 18, 25, January 1, 8. Amer. Nat., December. Amer. Geol., December. Botanical Gazette, December. Illinois Wesleyan Mag., October, November. Psychological Review, January. De Gids, January.

OBITUARY.

EMIL DU BOIS-REYMOND.

BORN NOVEMBER 7, 1818. DIED DECEMBER 26, 1896.

IN Professor du Bois-Reymond science has lost not only a great physiologist, but the veritable founder of one of the most important branches of physiology. Though not a German by descent he was born in Berlin, lived there nearly all his life, and was absolutely identified with its University and Academy of Sciences.

His father was originally a watchmaker in Neufchâtel, but had moved to Berlin, where he obtained a very good position. His mother was of Huguenot extraction. He began his education at the French College in Berlin, and passed thence to the College of Neufchâtel. But at eighteen he matriculated at the University of Berlin in the philosophical faculty, and after a year settled down to those studies which led up to his life's work—his medical curriculum lasting from 1837 to 1843. Johannes Müller—the grandfather of modern physiology, if such men as Ludwig and Du Bois-Reymond may be regarded as its fathers—then occupied the chair of physiology at Berlin, and to his influence some of the greatness of Du Bois-Reymond's future career may justly be ascribed. Matteucci's essay on the electrical phenomena of animals appeared in 1840, and at Müller's suggestion, Du Bois-Reymond undertook to repeat the observations which Matteucci had made. Thus began the work to which his life was afterwards devoted, and he chose the subject "Quæ apud veteres de piscibus electricis exstant argumenta" for his thesis for the degree of Doctor in Medicine. He became Assistant at the Anatomical Institute in 1844, and Privat-docent in 1846. The electrical manifestations of muscle and nerve, to the investigation of which he now applied himself with untiring ardour, constituted an unworked field, and Du Bois-Reymond was well fitted to explore it. His energy was unbounded, his mechanical skill as an experimenter unrivalled, and he displayed the greatest ingenuity in devising the requisite apparatus for his researches. To this day the Du Bois-Reymond induction coil and key and the non-polarizable electrodes which he invented are amongst the earliest apparatus with which the student of the physiology of muscle and nerve is confronted on commencing laboratory work. His discoveries soon brought him into a prominent position among physiological workers, and the publication of his "Researches on Animal Electricity" in 1848 and 1849

established his reputation. He strongly advocated the view that the phenomena of animal life were explicable on physical and chemical principles. In 1858, having already been Extraordinary Professor for some two years, he succeeded his great master as Professor of Physiology in the Berlin University, and held that post till his death —a potent influence for good upon all those who came under his influence, and the founder of an important school of physiology. In 1867 he was appointed Honorary Secretary of the Berlin Academy of Sciences, and this post he very worthily occupied, for he was no mere specialist in his own subject, but a man of wide culture and attainments, as his writings on other subjects testify. The magnificent Physiological Institute, which was built in Berlin after the war with France and which is one of the most complete and perfect in existence, was planned by Du Bois-Reymond, and here it was his satisfaction to witness the accomplishment of much admirable work by the younger men, Baumann, Fritsch, Kronecker, Goltz and others, who followed in his footsteps.

In person, he was of a medium height, but of strong frame, and with piercing eyes and a full beard. He possessed remarkable powers as an orator, and was master of a clear and lucid style in lecturing. Not only were his physiological lectures highly appreciated by students, but the weekly popular lectures on general subjects which he used to deliver at the University attracted very large audiences. As Secretary to the Academy of Sciences he delivered orations on the occasion of the death of any of the members, which will long be remembered. He was a man of the utmost frankness and sincerity and always stated his convictions in clear and emphatic language: though taking no direct part in politics, he had the reputation of being a Radical. Among his general writings may be mentioned the celebrated "Essay on the Limits of Natural Knowledge," essays on Physiological Teaching, on University Organisation, and on Civilisation and Natural Science. He wrote also on Leibnitz and on Voltaire in their relation to Natural Science.

In the domain of physiology Du Bois-Reymond remains for all time as the great founder of our knowledge of the electrical phenomena of muscle and nerve. As the direct result of this he played a foremost part in breaking up the vitalist doctrines which had previously held sway in physiology. Of all the bodily functions, muscular and nervous energy were held to be the most clearly "vital" in their nature, and when it was demonstrated that they were associated with and conditioned by definite and measurable physical and chemical changes, vitalism, or at least the old vitalism of our forefathers, received its death-blow. Du Bois-Reymond, indeed, would have nothing to do with the modern revivals of vitalism which have been propounded in recent years by Rindfleisch and others, though at the same time, as his essays shew, he by no means took up the narrow position that every problem must be regarded only from the physical

and experimental side. His greatness as a physiologist depended very largely on his exquisite skill and ingenuity as an experimenter. Some of his theories and views have been challenged, notably by Hermann, and it is pretty generally admitted that his "currents of rests" are rather to be explained as "currents of injury." Nevertheless, the great bulk of the new facts which he discovered in his own special sphere remains not only uncontroverted but confirmed by subsequent experimentation, and if some of the theories which he built on them have required modification, this can in no way detract from the greatness of his work as a pioneer in the field. In Du Bois-Reymond has passed away a true father of modern physiology, and one whose influence will be felt so long as the science shall endure.

DAVID ROBERTSON.

BORN (Nov. 28, O.S.), DECEMBER 10, 1806. DIED NOVEMBER 20, 1896.

WHEN David Robertson was three weeks old he lost his father. His mother was left with three little children and no external resources. At the age of eight the 'wee Davie' became a cowherd. At twenty-four, having had experience of many and varied employments, he took up the fancy that, instead of being a farm labourer, he might as well be a medical man. He was on the point of accomplishing this heroic resolve, when, just as he was ready to take his degree, his course was again diverted. He became a tradesman, a merchant, a man of substance, and eventually, at about the age of fifty-four, he relinquished the lucrative paths of commerce in favour of an ever-growing and engrossing devotion to the study of natural history. Thenceforward the observation of nature, with a special leaning to aquatic zoology and botany, became the business and the pleasure of his life. His researches were carried on in the most unselfish spirit. The keynote of his work was helpfulness. He might be diffident and over-anxious in publishing papers of his own, though these were numerous and excellent, but he was ever eager, confident, and enthusiastic when encouraging others, when showing hospitality to men of science, when searching for needed specimens, or when discovering and making known some 'happy hunting ground.' Fortunately, his wife, who survives him, was in full sympathy with his tastes and pursuits, and not only a sympathetic companion but a zealous and highly capable fellow-worker in the same field of science.

During the latter years of his prolonged and honoured life, Robertson's heart was much set on securing the permanence of the Cumbrae Station for marine studies. In a letter to the present writer, dated December 3, 1895, he says:—

"You will, I am sure, be pleased to hear that the prospects of the

Biological Station at Millport are continuing to improve. Dr. Reid, whom you saw at Fern Bank, has lately contributed £500 towards a new stone building for the Station. I do not remember whether I told you that the visitors to the Station last summer were over 6,000! and there have been a great many workers at the Station during early spring and the summer months, who all give excellent testimonials expressing their opinion that no better situation than Millport could be selected for a biological station. The purity of the water and the varied character of the bottom of the neighbouring sea, the richness of animal and vegetable life, the absence of strong currents, and well sheltered bays, make boating safe and pleasant, and where dredging is concerned it is more expeditious and agreeable, and not the least [advantage is] the large fleet of boats, and there is no place in the United Kingdom where the hire is so cheap—three pence per hour for a boat with oars.

"I have been reading the life of Darwin, by his son. He speaks often of his blessed pigeons and his pet pangenesis, and I fear my discourses will come to have an uncontrollable tendency to drift on to the Biological Station of Millport."

This summer he had the satisfaction of turning the first sod for the erection of the new building. In its completion and prosperous employment for the furtherance of science, Robertson will have a memorial more to his own liking than a monument in Westminster Abbey.

Besides being a Fellow of the Linnean Society and a Fellow of the Geological Societies of Glasgow and London, he received distinctions that were none of his own seeking, and that took his modest nature by surprise. At fifty-nine he found himself an honorary Fellow of the Imperial Royal Zoologico-Botanical Society of Vienna. The Natural History Society of Glasgow chose him for its President when he was eighty-one. At eighty-eight he was made an honorary D. C. L. of the University of Glasgow—a fit and graceful crowning in his native city, among friends who knew and loved him, of his memorable career. T. R. R. S.

THOMAS GWYN EMPEY ELGER, whose excellent handbook, "The Moon," we reviewed in August, 1895 (vol. vii., p. 138), died at Bedford on January 9th, at the age of 59. Besides being an astronomer of repute, he was the chief authority on Bedfordshire archæology, and his collection of implements, from palæolithic to Norman times, found in the gravels of the Ouse Valley, was one of value. He was intimately connected with the Library and the Literary and Scientific Institute of Bedford, also with the Archæological and Natural History Societies of the county. In his profession of civil engineer, Mr. Elger had laid down railways in Denmark, and between King's Cross and Edgware Road, London, and had been engaged in many similar works of importance.

JOHANN AUGUSTE STRENG, the celebrated German mineralogist and chemist, died at Giessen, on January 9, aged 67. He was assistant to Bunsen at Breslau, and in 1853 became teacher of Chemistry at Clausthal. From 1867 till 1895 he occupied the Chair of Mineralogy in Giessen University. He made microscopical investigations of rocks from Minnesota and worked on rocks from Harz and the Nahe, also, among other things, on felspars, and series of phosphates.

We regret to record the deaths of:—On November 4, Lieutenant E. D. YOUNG, R.N., commander of the Livingstone Search Expedition, and discoverer of the Livingstone range of mountains, aged 55; VIVIEN DE ST. MARTIN, renowned for his researches in ancient geography, at Paris, aged 95; DR. STRAUSS, Professor of Pathology at the Paris Medical School, noted for his writings on tuberculosis and cholera, aged 51; on October 6, in Passy, the entomologist Professor A. HÉNON, aged 74; Captain LUCAND, of Autun, an esteemed mycologist; on December 17, at Munich, Dr. JOSEPH V. GERLACH, formerly Professor of Anatomy at Erlangen, aged 76; on October 7, F. BENSELER, Inspector at Vienna University Botanical Garden, aged 65; Dr. LUIGI CALORI, Professor of Anatomy in Bologna University; Dr. R. ZOJA, who has done some work in experimental embryology, with his younger brother Alfonso, on September 26, at Nigazzo, by an accident; Dr. P. BINET, Deputy Professor of Therapeutics in Geneva University, and author of many works on the action of drugs, aged 41; Dr. LEONARD J. SANFORD, formerly Professor of Anatomy and Physiology in the Medical Department of Yale University, who died at Newhaven, on December 12, 1896, aged 64; F. HAZSLINSZKY, mycologist, described as the Nestor of Hungarian botanists, at Eperies, on November 19; on December 12, at Copenhagen, Professor ASGER STADFELDT, aged 66; F. SACCARDO, of Avellino, an authority on vine diseases, and a writer on lichenology, on October 6, aged 27; on October 15, in St. Petersburg, the Director of the Botanical Garden, Professor A. BATALIN; BARON N. DELLINGSHAUSEN, naturalist, at Riga; Dr. F. MORAWITZ, founder of the Entomological Society of St. Petersburg; Dr. MODEST GALANIN, editor of the St. Petersburg *Journal of Public Hygiene*; HORATIO HALE, the anthropologist, of Canada, on December 29; Dr. W. H. ROSS, formerly Professor of Anatomy in Mobile Medical College, Alabama.

Naturæ Novitates announces the deaths of: H. J. POSSELT, Conchologist at the Zoological Museum, Copenhagen, on July 20; A. NEGRI, Privatdocent in palæontology and geology at Padua, on December 11; S. SCHOLZ-ROGOZINSKI, African explorer, in Paris, aged 35.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

AT the Smithsonian Institution F. W. True succeeds the late G. Brown Goode as Assistant Secretary and Director of the National Museum, while Richard Rathbun becomes Assistant in Charge, in place of the late W. C. Winlock. E. Büchner, to be Director of the Zoological Museum of St. Petersburg Academy of Sciences, in place of T. D. Pleske, resigned; Augusta Ärnbäck, to be Amanuensis to the Professor of Zoology at Stockholm University; Professor M. J. Elrod, of Illinois Wesleyan University, to be Professor of Biology in the Montana State University; Clarence W. Perley, to be Assistant in Biology at the Massachusetts Institute of Technology; W. E. Castle, to be Instructor in Biology in Knox College, Galesburg, Ill.; Miss Knight, to be Professor of Anatomy and Pathology to the Lhudiana Medical School, India; G. Hedren, to be Amanuensis at the Polyclinic of Upsala University; J. H. Burkell, late Assistant Curator of the Herbarium, Cambridge University, to be an Assistant in the Kew Herbarium; A. Pestalozzi, to be Assistant in the Zürich Botanical Museum; Ernest Clark, Secretary of the Royal Agricultural Society, to be first Gilbey Lecturer in the History and Economics of Agriculture, Cambridge: Dr. Edler, of Göttingen, to be Associate Professor of Agriculture, at Jena; Professor Blass, to be full Professor of Geology at Innsbrück; Dr. Szadeczky, to be Associate Professor of Geology at Klausenberg; F. B. Peck, of Amherst College, Mass., to be Associate Professor of Geology and Palæontology at Lafayette College, Easton, Pa. During the absence of Professor H. O. Lindgren, of Lund, on parliamentary duties, Professor C. M. Fürst will lecture in Anatomy, and Dr. N. E. Wadstein on anatomy and histology; during Professor S. A. B. Lundgren's absence from the same university, the teaching in geology will be conducted by Dr. J. C. Moberg.

THE only New Year's honour in which the British public has shown much interest has been the raising to the peerage of Sir Joseph Lister, who will, we are glad to learn, be still known under his own famous name, as Lord Lister.

THE Royal Order of the Prussian Crown of the second class has been given by the Emperor of Germany to Dr. Roux, whose co-discoverer of the antidiphtheritic serum, Dr. Behring, has been invested with the Grand Order of the Crown of Italy. Other honours are the gifts by the Czar of the Cross of the Commander of the Order of St. Anne to Mr. Gérard, Director of the Paris Municipal Laboratory; the Cross of St. Stanislas, to Dr. Bordas, Sub-Director; and the same to Dr. Bertillon, Director of the Anthropometric Service.

WE learn that Professor Otto M. Torell, head of the Swedish Geological Survey, is seeking retirement on a pension.

THE Walsingham Gold Medal of Cambridge University has been awarded to W. McDougall, of St. John's College and St. Thomas's Hospital, for original researches in physiology.

R. W. T. GÜNTHER has been elected to an Official Fellowship at Magdalen College, Oxford, as tutor in natural science.

THE late Dr. H. F. Antells left to the University of Helsingfors the sum of £40,000, the income from which will be applied to travelling scholarships and scientific expeditions.

ALFRED NOBEL, the discoverer of dynamite, has, after making a few personal bequests, left to Science as his residuary legatee, a fortune of some £2,000,000. We give a translation of the passage in his will relating to this :—"This is set apart as a prize for those who, during each preceding year, have done the greatest service to mankind. The income is to be divided into five equal parts as follows :—a part to him who made the most important discovery or invention in physics ; a part to him who made the most important chemical discovery or improvement ; a part to him who made the most important discovery in the domains of physiology or medicine ; a part to him who produced the most remarkable work in the field of imaginative literature ; and a part to him who worked most or best in uniting the nations and in doing away with or diminishing standing armies, as well as in founding or spreading peace congresses. The prizes for physics and chemistry are to be awarded by the Swedish Academy of Science ; for physiological or medical works by the Carolinska Institut of Stockholm ; for literature by the Academy of Stockholm ; and for the pioneers of peace by the Norwegian parliament. It is my express will that in the award of the prizes no regard shall be paid to nationality, so that the most worthy shall receive the prize, whether he is a Scandinavian or not." To criticise this more than princely gift is ungracious, but we cannot help thinking that prizes of this nature do not encourage honest and profitable work so much as do the endowment of institutions or the foundation of research-scholarships.

BARON HIRSCH bequeathed 2,000,000 francs to the Pasteur Institute, which will be enlarged by chemical and biological laboratories.

THE Sedgwick Memorial Museum of Geology at Cambridge is to be built on he ground lately belonging to Downing College. We understand that the plans (published in NATURAL SCIENCE, vol. iii., p. 451, Dec., 1893) will, in consequence, have to be altered.

A NEW departure has been made this winter at the Science and Art Museum, Dublin, in a series of Museum demonstrations, undertaken by members of the staff and other helpers. Two demonstrations a week have been given through December and January, natural history alternating with art subjects. The difficulty of exhibiting small museum-specimens to a large audience led to a restriction of the number of tickets issued for each occasion to thirty, or at most fifty. It is satisfactory to record that there was a large demand for tickets, and that the audiences seemed thoroughly interested with the explanations of the objects.

ON January 11 a gas explosion in the Public Buildings at Penzance considerably shattered the Geological Museum. Dunipace House, near Larbert, the residence of Mr. J. A. Harvie Brown, has been destroyed by fire, including the museum, which contained one of the finest private collections of stuffed birds, skins, and eggs in the country.

THE Report of the now municipal Museum of Bristol, presented in December, 1896, shows that the repair of the premises has been continued, and that a number of the cases on the ground floor have been renovated and their contents better displayed. In this way the anthropological department has been set in order, to the gratification of the public ; the rock collection has been rearranged and newly labelled. A nearly-perfect skeleton of the moa, presented by Mrs. Tuckett, has been set up in the geological department. We have already noticed the students' geological collection of about 1,000 named specimens that has been fitted up. An herbarium of European plants, with accompanying literature, has been presented by Mr. W. G.

Smith. The attendance during two years was 160,000, of whom no less than 4,225 visited the museum on Whit Monday, 1896. In the library an important move ha been the collection in a separate room of all books and pamphlets connected with Bristol, their number now reaching 2,129. It is also satisfactory to learn that the excellent penny guide to the museum by Mr. E. Wilson meets with a ready sale. We have, indeed, already received a copy of the seventh edition, issued September. 1896. It has a view of the museum as frontispiece.

THE East Wing of the American Museum of Natural History at New York was opened on November 30, and excellent illustrations are contained in *Science* of December 11. With the West Wing now in course of erection, the museum will have a total floor-space of 148,258 square feet. The twenty-seventh annual report states that a deficit of nearly £1,600 was met by special subscription from the trustees. The collections have been largely augmented from the various expeditions sent out. Two new halls have been opened, one of Ethnology and the other of Vertebrate Palæontology. The collection in the latter, up to the summit of the Oligocene, must be indeed unique if it contains, as asserted, with few exceptions, more or less complete remains of every genus and species known. The south side is occupied by the Perissodactyla, Rhinoceroses, and horses; the north by the Mesozoic animals, the meseutherian Amblypoda and Creodonta, the Tillodontia, Rodentia, Insectivora, and Carnivora. In the ethnological department the material is placed in geograpbical order; where the collections are sufficiently complete the following subjects are illustrated: Physical types, relation of man to nature, industries, dress, trade, social organisation, and religion.

THE annual *Report* of the Auckland Institute and Museum for 1895-6 shows that the revenue of the museum is likely to be increased, through some of the Crown lands from which its income is derived having been taken up under mining leases. The museum appears to suffer from the want of a taxidermist, which prevented its acceptance of such important specimens as an adult leathery turtle and a fine example of the true sun-fish, *Lampris luna*. The Ethnology Hall has received from the Chief Hami Te Waewae the gift of a carved post or "tiki" from Ota Kanini Pa, Kaipara, a fortress well known in ancient Maori history. It will be remembered that some time ago Little Barrier Island was set aside as a preserve for the rarer members of the New Zealand fauna. The *Report* states that there are persistent rumours as to the slaughtering of the birds for sale, and in consequence of this all Maoris have been removed from the island.

A BOTANICAL INSTITUTE in connection with the Garden has been established at Münster, Westphalia.

THE Castle Garden Aquarium at New York was opened on December 10, with a collection of about 100 species of fish, which will be increased by importations.

AMERICAN botanists have taken up very eagerly the suggestion made by Professor D. T. MacDougal that a tropical laboratory for botanical work should be founded by them. The situation proposed is either the East Coast of Mexico or an island near the Caribbean Sea.

THE Belgian Government has instituted, in the International Exhibition at Brussels in 1897, a Scientific Section. This will be devoted to Physics and Mathematics and the Natural Sciences, with a department for Bibliography, a subject much cultivated by our Belgian colleagues at present. The president of the section will be General J. de Tilley, while Mr. Eugene van Overloop will act as

Commissary for the Government; their secretaries will be Messrs. G. Gilson and E. van den Broeck. Mr. A. Houzeau de Lehaie will be president of the Geological and Geographical Section; J. B. Carnoy of the Biological; Dr. E. Houzé of the Anthropological; and Dr. F. van der Haeghen of the Bibliographical Section. A large number of prizes are offered for best collections and answers to certain questions, full particulars of which can be obtained by writing to one of the secretaries.

THE "Allgemeinen Gartenbau-Austellung" at Hamburg will hold an independent scientific exhibition between May 28, 1897, and the end of September. There will be shown: (1) Diseases of cultivated plants due to mechanical, atmospheric, or soil conditions; (2) animal and vegetable enemies of plants; (3) ditto, friends of plants; (4) variations and abnormalities in plants; (5) comparative manurings of pot-plants; (6) wild ancestors of cultivated plants; (7) living, exotic, useful plants in pots; (8) a selection of the same preserved; (9) selection of plants and plant-structures arranged on morphological and biological lines; (10) the results of scientific pollinisation experiments; (11) scientific aids to gardening. The programme will be sent to interested applicants by Dr. R. Kraepelin, Naturhist. Museum, Hamburg.

THE Committee of the International Geographical Congress, held in London in 1895, has recently sent to the various geographical societies resolutions urging the importance and desirability of: (1) Antarctic exploration; (2) a geographical bibliography, compiled by various states; (3) a topographical survey of Africa; (4) a map of the earth on a scale of 1 : 1,000,000, with the meridian of Greenwich and metric measurements; (5) the continuance of physical investigations lately made in the Baltic, North Sea, and North Atlantic; (6) an international system of seismographic stations; (7) agreement between the various geographical societies as to the spelling of foreign names; (8) the printing on all geographical maps henceforward the date of their publication. Further, they request the opinion of the societies as to the application of the decimal system to the measurement of times and of angles.

THE Geological Society of London has made its awards for this year as follows:—Wollaston Medal, W. H. Hudleston; Murchison Medal and part of fund, H. B. Woodward; Lyell Medal and part of fund, G. J. Hinde; Bigsby Medal, Clement Reid; Wollaston fund, F. A. Bather; balance of Murchison fund, S. S. Buckman; balance of Lyell fund, Joseph Lomas and W. J. Lewis Abbott.

THE Surveyors' Institution is building itself a new home in Great George Street, Westminster, from plans by Mr. A. Waterhouse. The second floor of the building is devoted to the museum of forestry, which is being greatly elaborated by the Institution.

WE learn from the *Revue Scientifique*, and why we should be left to learn it from this we do not quite know, that a mycological society is being formed in England under the presidency of Mr. G. Massee. Excursions of a week's duration will be taken each year, and the first, beginning on the third Monday in September, 1897, will be to the Sherwood Forest.

THE naturalists of Manchester are turning their attention to interesting points in the history of their societies. The Literary and Philosophical Society has published as complete a list as possible of its members since its foundation more than a century ago. The President of the Geological Society of the same city (Mr. Mark Stirrup), now sends us a copy of his address on the early history of that body. Next to the Yorkshire Geological and Polytechnic Society, this is the oldest provincial geological society in England, having been founded in 1838. During its long career it has not only continued to be a flourishing organisation for the promotion

of research, both in pure science and in the application of geology to mining, but it has also contributed very largely to the fine collection of geological specimens in the Manchester Museum, now in the Owens College, of which it provided the nucleus many years ago.

MR. R. L. JACK sends us an unpaged (? unpublished) extract from vol. xii., *Proc. Roy. Soc. Queensland*, announcing the discovery by Mr. J. Coghlan, of two undoubted specimens of *Orthoceras* in the Cairns Range of Western Queensland. This appears to prove the extension of Ordovician rocks from S. Australia. Hitherto the oldest fossiliferous rocks known in Queensland have been the Burdekin Beds of Middle Devonian age.

WE understand that important results have been obtained by Mr. J. E. S. Moore, who has been investigating the fauna of Lake Tanganyika. His collections seem to show that it cannot be very long, geologically speaking, since this body of water, now fresh, formed a part of the sea.

CAPTAIN BOTTEGO has reached Lake Rudolf, and is on his way back to Mombasa. Mr. A. H. Neumann, who has returned to England also from Lake Rudolf, followed the eastern shore into the Randile country. He will present some of his spoils to the Brisish Museum (Nat. Hist.).

G. KOLB, a Bavarian explorer, has, during the past two years, made two journeys in the country between Kilimanjaro and Kenia in Africa. On his way to the latter he passed the Gunga Lake, situated in a crater and surrounded by steep walls; though 6,500 feet above sea-level, it is inhabited by hippopotami. Attempting the ascent of Kenia from that side, he found a large oval plain, thinly covered with grass; at one end was a glacier brook, round which the temperature dropped at night to 9·5° Fahr.

THE following details about French explorers are gleaned from *L'Anthropologie*. Mr. Clozel, Administrator of the French Possessions on the Ivory Coast, is endeavouring to make valuable ethnographic and geological collections. Important results are expected from two such enthusiastic explorers as Messrs. Bonnel de Mézières and de Béhagle, who are starting for Central Africa. Mr. Bonin has returned to Tonkin from the south-western provinces of China, whence he brings much material and many facts of ethnographical and anthropological nature. On their way from Turkestan to Siberia, Mr. Chaffanjon and his party have gathered large collections of the fauna and flora, and accumulated much information regarding ethnography and geography. In Siberia, too, Baron de Baye has been carrying on his archæological and ethnographical studies. Mr. E. Blanc, who has been to Nijni-Novgorod, is bringing back rich scientific collections. Mr. Raoul, official Colonial chemist, is starting on Government business for Borneo, where he hopes to carry on scientific studies. The Hourst expedition, whose return was noted in our last, has proved the navigability of the Niger from Bammako to the sea.

REUTER states that Messrs. Oloufsen and Philipsen, two Danish officers, have returned from portions of the Pamirs hitherto unexplored by Europeans with rich collections for the Natural History Museum, Copenhagen, over 300 photographs of places and racial types, and valuable meteorological observations, some from 14,000 feet above sea-level. Some of the tribes met with were of diminutive stature, their domestic animals being small in proportion; they are fire-worshippers, have no money, and buy wives for five or six cows, or fifteen sheep, apiece.

THE University of Washington at Seattle sent a natural history expedition, consisting of H. H. Hindshaw, Mrs. Hindshaw, and T. C. D. Kincaid to the Snake River, and its results are given in the *American Naturalist* for December. The sand-

hills of Douglas Co. are, in part, covered with a flow of basaltic lava, and form the water-bearing rock of Eastern Washington, known as the John Day bed. This contains many mammalian bones, large collections of which, as well as of insects and Indian relics were obtained, and are now placed in the museum of the University.

THE *Daily Chronicle* of January 18 announces that on January 14, Zurbriggen, the famous Swiss guide attached to Fitzgerald's Andes expedition, reached the summit of Aconcagua, over 24,000 feet above sea-level. Fitzgerald reached the aréte at 23,000 feet.

According to the *Daily Chronicle*, preparations for the Austro-German Antarctic Expedition have begun. Julius von Payer is taking a leading part. A preparatory expedition will start from Melbourne next summer with the object of wintering at Victoria Land, and making preliminary investigations. Dr. Meves (Stuttgart) is entrusted with the geological and geographical researches, and Dr. Schöner with those relating to botany and zoology. The final expedition is to be fitted out with two ships, which will sail as far as the meridian of Kerguelen Islands. One is to winter there to serve as a refuge for the crew if the principal ship founders. The latter is to advance as far as the Antarctic magnetic pole, which has hitherto been fixed only by means of a calculation.

KING LEOPOLD of Belgium offers a prize of £1,000 for the best essay sent in before the 1st of July next on the meteorological, hydrological, and geological conditions of Equatorial Africa, from a sanitary point of view. The chief points to be discussed are the mode of life, work, food, clothing, dwelling, most favourable to health, the conditions affecting the life of Europeans in the Congo basin, and tropical diseases and their treatment.

MR. ALFRED SHARPE, the Acting Commissioner in British Central Africa, has issued regulations with regard to the new game preserve on the Shire River. A written permission must be obtained before shooting or trapping animals within a prescribed area, licences may be refused, and breaches of the regulations punished.

ON Monday, December 28, a large bog near Rathmore, about twelve miles from Killarney, situated on a table-land about 776 feet above sea-level, burst its bonds, and flowed, in the form of black, liquid mud, over a large part of the surrounding country, destroying several habitations and lives. The course followed that of the Ownacree river. The escape of the bog had no connection with any earthquake shock, but seems to have been due to recent heavy rains, which loosened the lower layers of the bog, and caused rifts in the upper layers.

WITH reference to the plague now raging in Bombay, the *Bombay Gazette* states that the medical officer, Mr. Hankin, ascribes its diffusion chiefly to rats and ants. Infected rats deposit the disease-germs on floors over which they pass. When they die their bodies are eaten by ants, which absorb the bacilli and deposit them in cracks, and especially near water. It is therefore not enough to remove patients from their homes: it is the healthy that should be removed and carefully segregated.

FROM a letter in the *Shooting Times* of January 9 we learn that anthrax has broken out among the elks in the forest of Ibenhorst, near the Kurisches Haff, in the north of Germany. This, says the correspondent, R. Zeitler, is a heavy blow for the stock of elk in Germany, since about forty elks perished in Lithuania last spring in consequence of the high water and of frost. More than usual, also, were shot in the autumn. To preserve at least a small stock in East Prussia, it has been proposed that no elk should be killed during the next two years, that in future no cow or calf should be shot at any time, and that the male elk should be shot only during a four week's season.

CORRESPONDENCE.

"A Plague o' both your Houses!"

Under this appropriate [?] heading Natural Science (vol. x., p. 4) makes the statement:—"Our need still is for facts . . . we would suggest that the present is not the time for speculation on the merits or failings of Natural Selection. . . . A truce to library papers for ten or twenty years! . . . Perhaps, at the end of the suggested period, it may be possible to formulate important results based on experimental evidence, say, for instance, whether variation is indefinite or definite."

As you say that you were "urged to the above remarks" by my paper:—"Does Natural Selection play any part in the Origin of Species among Plants," perhaps I may venture to say that I *have* done, and that, too, for *twenty* years, precisely what you now advise me to begin to do! As you say "no new experiments were advanced" by me, I can only presume that you have not done me the honour to read my book on "The Origin of Plant Structures," in which plenty of experiments are recorded.

I would also like to observe that it is no question of "speculation" or of pitting one *theory* against another, for the conviction that *definite adaptation* is all-sufficient to account for the origin of species, is forced upon me by the accumulation of positive facts, which speak for themselves. No *theorising* is required at all. The origin of species by means of natural selection is still, as I pointed out, an unverified *deduction*; whereas adaptation by definite variations is proved, not only by a vast accumulation of parallel series of facts, *i.e.*, by *induction*, but has been verified abundantly by experiments. If this be not sufficient to convince Darwinians, I venture to think that nothing ever will. Conversely, until they can prove the universal existence of *indefinite variations*, The Origin of Species by Means of Natural Selection must remain a baseless hypothesis.

George Henslow.

[We have not only read but reviewed Mr. Henslow's book (vol. viii., p. 201), nay, more, we recommended it. We implied in the Note of which Mr. Henslow complains, that in his paper at the Linnean he advanced no new experiments. If he did adduce any fresh experiments conducted by himself since February, 1895, we will admit our error, and shall be pleased to publish the details of them.—Ed., Nat. Sci.]

Natural Science for March, 1896, alluded to Dr. J. D. F. Gilchrist's method of killing *Aplysia* in an expanded condition, by cocaine. In May, 1896, Mr. H. Hanna wrote to us that a 1 per cent. solution of chromic acid was "simply perfect" for the purpose. Dr. Gilchrist now writes to us from Cape Town as follows:—"I have, at Naples and elsewhere, repeatedly used a 1 or 2 per cent. solution of chromic acid—the method at present in use at Naples—and, after many trials with this and other methods, have come to the conclusion that the method I recommend is the most satisfactory."

NOTICE.

To Contributors.—*All communications to be addressed to the* Editor *of* Natural Science, *at* 22 St. Andrew Street, Holborn Circus, London, E.C. *Correspondence and notes intended for any particular month should be sent in not later than the* 10*th of the preceding month.*

To the Trade.—Natural Science *is published on the* 25*th of each month; all advertisements should be in the Publishers' hands not later than the* 20*th.*